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NOTES AND COMMENTS.

The Preliminary Notice.

THERE is, we are gravely assured, much to be said for the Preliminary Notice. This much is usually said by two classes of people: first, the priority-hunter; secondly, the type-hunter. The priority-hunter is the man who is, as a rule, more concerned with adding a new name to the literature of science—to which name his own, as he fondly supposes, is to be attached for ever—than he is with investigating the structure and relationships of the species which he forces upon an unwilling world. He lives in a perpetual fear lest some colleague shall anticipate his work, and finds it necessary to rush his driblets of papers through the press, with but scant attention to the soundness of their workmanship, and with little care that they shall prove intelligible to his readers. The type-hunter is usually a museum official. In common with the officials of other museums, he one day receives some specimens from a distant country. He imagines it to be his duty to the institution which he serves to fly to the printing-office with a hastily-composed description of those among them that he believes to be new species, in order that those specimens may acquire the fictitious value of what are known as "types." His business, like that of the priority-hunter, is not with his scientific colleagues; he cares little whether his description can be understood either by his fellow-workers or by future generations, for, to do him justice, he usually excuses his action on the ground that a museum catalogue, or a Government report, or some other ponderously moving publication, will eventually provide the world with all the details for which it is anxious. Even if the monograph never appears, it matters little to him; the museum has the type, and, in the absence of proper figures and description, this renders it all the more necessary for students to visit his museum. Hence, fame to the museum, and promotion to himself as an active official! But these two classes do not, we earnestly hope, represent either a baser majority or an enlightened minority of scientific workers. Rather, we venture to affirm, do they constitute a minority which we

prefer to leave without epithet. In a fair-minded discussion their arguments should not be given undue weight. From the point of view of pure science the only advantage that can be ascribed to the preliminary notice is that the writer of it may possibly have his mistakes corrected and additional information given to him before he commits himself to quarto form. But a worker in love with his subject is usually in sufficient touch with his colleagues to obtain criticism, advice, and information through the simple medium of the Post Office, without airing his half-baked opinions before the world at large.

On the other hand, the charges that may be brought against the preliminary notice are varied and weighty. It affords undoubted inducement to a writer to scamp his work, since he salves his conscience with the thought of that wonderful and richly illustrated paper that is to establish his reputation—some day. Meanwhile, his readers are presented with statements of irritating insufficiency and of doubtful validity, excused, perhaps, by some such remark as that the periodical in which they appear is "not a permanent medium of publication," and that those people who, owing to their private sources of information, are able to understand the statements, find this preliminary publication of considerable service. The less fortunate public wishes that such people would be content with their private sources of information. Though the foundling be cast upon the world, there is still much fear that it may never reach maturity. Countless accidents may prevent the publication of the completed work. There are, indeed, instances of preliminary notices that have been calmly thrown over by their reputed parents, owing to some doubt cast upon their legitimacy. Other notices, again, when they have served their turn, are not alluded to in the final monograph, an action that is distinctly ungenerous. As a result, even the priority-monger becomes involved in some confusion, for a species may be introduced as new, although the curious bibliographer knows that it was really published two years ago. But two years is a long time, and it is often found that names have undergone such transformation in the interval that the connection between the preliminary and the final form is hard to recognise. Even in many cases that do not deal with species or their names, but with presumed additions to our knowledge of scientific facts, it is eventually found that the chief result of the preliminary notice has been to start a number of errors upon their eternal career.

If, after all that has here been urged, it still be maintained by any of our readers that under certain circumstances the preliminary notice is a necessity, let us at least ask that the statements published in it shall have been verified with as much care as those of the final monograph, and that any new species therein introduced shall be provided with diagnoses that are not merely diagnostic, but also intelligible to every worker. Even fossils, whether from the Eocene of the

Middle Atlantic slope or from other better known formations, are to be regarded as something more than counters for the stratigraphical geologist to gamble with. We will not insist upon illustrations, but if these are not given it is all the more necessary that the description should clearly convey the appearance of the species. Still, though we make this concession, we urge again that what is really wanted nowadays is careful and detailed work, accompanied by a thorough revision of all the imperfect and inaccurate statements launched upon the world, chiefly in the form of preliminary notices.

The Variation of Yeast-Cells.

The *Annals of Botany* (vol. ix., no. 36) contains a short account of some experimental studies on the variation of yeast-cells by Professor E. C. Hansen, of Copenhagen. Professor Hansen had already shown that the form and size of the cells cannot be used alone, as was done by Reess, to characterise species; for from each of the species it is possible to produce the rest by varying conditions of cultivation. Thus the large oval cells, characteristic of *Saccharomyces cerevisiæ*, the beer-yeast, under favourable circumstances of nutrition, may be developed by wine-yeasts with small oval cells, Reess's *S. ellipsoideus*, and the converse change may also be effected. Again, the sausage-shaped cells of *S. Pastorianus* can be produced in several ways from the other two yeasts. There are many reasons which make it probable that the oval form is the primitive.

Hansen has also shown that cultivation for a long time in aërated wort, at a temperature above the maximum for spore-formation and approaching that for vegetative growth, causes a complete and permanent loss of the powers of spore-formation, and of producing films on the surface of liquids. The loss of the latter property causes a marked alteration in their influence upon the liquids in which they grow; the cells of the film which develops on fermented beer-wort cause the liquid to become lighter in colour, and also produce a vigorous oxidation by which the alcohol is broken up into carbonic acid and water. Thus, while an ordinary yeast-culture, left to stand for six months, contained only 1·5 per cent. of alcohol in the fluid, that of a non-film-producing variety obtained from the same stock showed an alcohol percentage of 5·5, that is, the same amount as at the end of the first month. By cultivation on the surface of nutritive gelatine, varieties were developed having a greater fermentative power than their primitive forms, in one case amounting to the production of as much as 3 per cent. more of alcohol. Another experiment which has an economic bearing shows an effect of the chemical composition of the nutrient liquid. *S. Pastorianus* is one of the disease-yeasts of beer, imparting an offensive odour and a disagreeable bitter taste. When cultivated for a number of generations in a solution of cane-sugar in yeast-water, a growth was obtained which for a time had lost these properties. Frankland and

others have proved that when bacteria are cultivated in a certain way they may lose their special fermenting powers. Hansen, however, was unable to demonstrate a similar behaviour in the case of alcoholic yeasts. Their cells may be temporarily much enfeebled, and varieties may be produced yielding less alcohol than the primitive forms, but, hitherto, it has been impossible to produce one which has completely lost its power as an alcoholic ferment.

A propos of the possibility of replacing the yeast of cultivation (*S. cerevisiæ*) by some of the species occurring in nature, with a view to obtaining new or better products, Hansen emphasises the fact that we know nothing of the history of existing supposed variations from a primitive form; for "we have never been able to carry one single Saccharomyces species back to its progenitor." The question as to whether the species are mere developmental phases of various common mould-fungi must still be considered an open one. Since, however, we now know many moulds that develop alcoholic fermentation fungi, it would not be surprising if some should be found to develop yeast-cells with the characteristic internal spores.

As regards the causes of the variations, Hansen finds that there are three important external factors, the nutritive substratum, aëration, and temperature. As regards the two former, it is only required that they should allow of a vigorous multiplication, and the range in which they may vary is a wide one. But as regards temperature, one or two degrees too much or too little is enough to prevent the changes. If the temperature is a little too low the effect will not be sufficiently marked, if too high, the multiplication of the cells ceases too soon, and the change that has begun does not become fixed.

The Nutritive Materials in Wheat-grains and Wheat-embryos.

It is a fact familiar to students of botany that the seeds of flowering plants correspond, not to an egg that has just been fertilised, but to an egg within which the chick-embryo has proceeded some little distance in its development, and has then for a time become quiescent. In some seeds, such as, for instance, the common broad-bean, the young embryo has absorbed the whole of the food-material which had been provided for it, and has packed that into its young seed-leaves. Thus, the part of the bean or of a walnut which one eats is the actual embryo. In other seeds, as for instance wheat-grains, the embryo remains very small until it germinates, and the part which is ground into flour and used as food is the endosperm or food-store, not yet absorbed by the embryo. In the number of the *Annals of Botany* referred to in our last paragraph, Mr. O'Brien gives the results of investigations he has made into the composition of the endosperm or food-store, and the germ or embryo of wheat-grains. The embryo is richly stored with aleurone-grains, thus contrasting with the endosperm, in which the proteid material is stored as gluten.

No starch is present, but there is a small amount of sugar and much oil. A further analysis of the germ-proteids showed that globulins and proteoses existed as in the endosperm, but that the gluten was replaced by soluble albumen.

Polyembryony in a Lily.

Mr. E. C. Jeffrey, in the same number of the *Annals*, describes an interesting case of polyembryony in a liliaceous plant, *Erythronium americanum*. It resembles the polyembryony which is so frequent in gymnosperms in the fact that the several embryos, two, three, or rarely four in number, originate from a single egg-cell. After fertilisation, which occurs in the normal way, the egg-cell forms a mass of embryogenic tissue, from the lower part of which several rudimentary embryos grow out. Ultimately, however, as in the gymnosperms, all except one perish, which in the seed is attached to a large, broad suspensor, the remains of the embryogenic mass.

Mr. Jeffrey's discovery adds another form of polyembryony to those already described in the order Liliaceæ. In *Funkia* and *Nothoscordum* Strasburger showed that the multiplicity of embryos arose from adventitious buds derived from the nucellus growing into the embryo-sac after fertilisation of its egg-cell. Early last year Tretjakow published an account of the formation of embryos from the antipodal cells in *Allium odorum*. Two modes of origin are distinguished by the present writer, *extrasaccal* and *intrasaccal*, from their occurrence outside or within the embryo-sac, that described in *Erythronium* coming under the second heading, where also must be included the case, described by Dodel and Overton, of *Iris sibirica*, where polyembryony is said to arise from fertilisation of the synergids.

The "Revue Scientifique" and Bibliography.

We observe that the *Revue Scientifique*, in its issue for December 28, 1895, announces that it will adopt the system of heading its articles with their proper decimal indices. A very lucid explanation of this method of bibliographic classification is given by the editor, Dr. Charles Richet. He concludes his article by saying, "Nous serons fiers d'avoir été les premiers à l'appliquer, en Europe, après MM. Lafontaine et Otelet." We are sorry that a journal which has done such good work in the cause of scientific bibliography as has the *Revue* should have been forestalled by another journal, even though that be Natural Science. We venture, however, to point out to Dr. Richet that the system was put into force in our January number, which was in the hands of the public on Monday, December 23, 1895, whereas it did not appear in his paper until January 4 of the present year. The learned editor of the *Revue Scientifique* will doubtless join with us in the hope that we may not be the only amicable rivals.

THE BIBLIOGRAPHIC CONFERENCE.

WE have at intervals alluded to the schemes of the Royal Society for the preparation of a complete catalogue of scientific papers, from the year 1900 onwards, classified under both authors and subjects. We have retailed to our readers such information as we could glean from our foreign contemporaries, or as was supplied to us by our foreign correspondents. From some papers which the Royal Society has at last kindly supplied to us, and from the recent address of Lord Kelvin, a few additional facts may now attain the publicity they deserve. Through the Foreign, India, and Colonial Offices invitations have been sent to the Governments of the twenty-one nations that are engaged in scientific work, of India, and of our colonies, to send representatives to a conference to be held in London during the first half of July next. It is proposed that the representatives appointed should communicate beforehand with the Royal Society, and make suggestions, by the help of which, as well as of those already received in preliminary correspondence, the President and Council might draw up some outline scheme which could serve as a basis for discussion, both before and during the conference.

In the original letter, dated 1894, which asked scientific bodies and individuals for their opinions, the scope of the catalogue was confined to scientific literature. This letter produced favourable replies. In the letters of August 15, 1895, it is proposed that the catalogue "shall be as complete as possible, in respect to all papers and other publications and works relating to pure and applied science." This extension will, without doubt, treble or quadruple the task, which even before was of appalling magnitude, and it is not surprising that it has—so our foreign correspondents inform us—met with strenuous opposition. We learn, for instance, that the Berlin Academy has signified its strong disapproval by declining to send a representative to the Conference, and that the Swedish Academy is not unlikely to follow suit. Doubtless the inclusion of engineering, medicine, and sanitation will gain financial support from practical men; but it will enormously increase the cost, and pure science may get stranded on the way. Certainly it is hard to see what the Berlin and Swedish Academies, or even the Royal Society, have to do with drains, electropathic belts, or liquid manure. Patentees and purveyors of such goods are able to take care of themselves. But surely the objectors would do better to give their arguments first and to retire afterwards. The object of a conference is to promote criticism, and to sulk in a corner is hardly a courteous response to a courteous invitation.

Among the details to be discussed at the Conference are: the question of language, for which English appears to have the suffrages of the majority; the mode of quoting titles, which most people wisely think should be in the original language, though a few would restrict them to English, French, or German, and perhaps Italian; the

manner of publication, for which the system adopted by the Bibliographical Bureau for Zoology is warmly advocated. It is, we suppose, probable that, with regard to the subject-index, the claims of Decimal Classification on the Dewey plan will be strongly urged, although we understand that the authorities of the Royal Society are themselves unfavourable to the scheme. Whether it meets with a welcome at the Conference will doubtless depend on the number of practical bibliographers and experienced cataloguers whose presence has been invited. If the Decimal method be adopted, the work of classification in the subject-index will be enormously facilitated, and if the example of Natural Science and the *Revue Scientifique* be followed by other publications, the work might be entrusted to the very young ladies who have lately been invading the public scientific libraries of London on behalf of the Royal Society's Catalogue. We believe, however, that another system, more understanded of the people, though more cumbrous and less certain, has lately been proposed by the Royal Society. This is that short abstracts, or detailed contents, of the contained papers should be inserted in each issue of a periodical. To this end certain societies have, we understand, been approached. Anything that will aid research is to be welcomed; but it may be pointed out that an abstract in French or Japanese, though more readily intelligible (in France or in Japan), is not so universally intelligible as the Decimal Index-number will, it is thought, before long become. Moreover, if the Conference decides to adopt the latter system, already in work in a large number of libraries and bibliographic centres, then the Central Office, when it begins its own labours, will be able, without friction, to use those of the many disinterested toilers in the same field.

We have made one or two suggestions, because suggestions are sought; and it is possible that those made by practical journalists, who have devoted much study to the subject, may be as valuable as those made by scientific investigators. In any case, this attempt is so great and so important that it is the duty of everyone who has seriously considered the question to speak out his honest opinion, without waiting till he is asked for it.

Sir Joseph Prestwich.

In the New Year's list of honours no award could have given greater pleasure to men of science than the knighthood conferred on the *doyen* of British geologists. Prestwich's first geological paper was read in 1834, and he is still writing. Before 1840 he had made his mark by discoveries of fish remains in Banffshire, and by a masterly monograph on the Coalbrookdale coalfield. His series of papers on the Tertiary deposits of the London and Hampshire basins, issued in 1846–1847, placed him in the front rank of British geologists; they settled for ever the main facts in the correlation of these deposits, and the accuracy of the observations has never been

questioned. In 1857, Prestwich announced the memorable discovery of marine Pliocene deposits on the summit of the North Downs; this conclusion was long denied by the officers of the Geological Survey, but, as in most cases, Prestwich's view finally gained universal acceptance. In 1860, he placed the question of the antiquity of man on a new footing, by his memoir on the implement-yielding gravels of Amiens and Abbeville. This was the first of Prestwich's classical papers on the drifts, a subject on which he had first written in 1851, and one which has ever since been his favourite. Most of Prestwich's work was done in the intervals of business life, and it was not till 1874 that he was appointed Professor of Geology at Oxford, where he taught till 1888. During his stay there he compiled his great Text-book of Geology, a work rich in suggestions, though unfortunately so behind the times in two departments that it has never exercised so wide an educational influence as it otherwise would have done. After Professor Prestwich had left Oxford for his picturesque home in the gorge of the Darenth, he returned with enthusiasm to his old love—the English gravels. In 1890, he issued his series of papers on the "Westleton Shingle." In these his genius for summarising complex controversy and for picking out the fundamental facts from a bewildering crowd of trivial and apparently contradictory observations enabled him to build up a theory which has gradually gained ground, and will certainly be the starting point for all future work. In the following year, he took up the defence of the rough flint implements found by Harrison on the chalk downs of Kent, a discovery almost as memorable as that of the implements in the gravels of the Somme. Until Professor Prestwich's defence of the claims of these stones to be regarded as shaped by man, the discovery was not accepted, owing to the startling alterations rendered necessary in the interpretation of the geology of the south-east of England. In some later papers, Prestwich has taken to deluges, with results unfortunate for his reputation among many younger English geologists. But those who regret most that he has, perhaps unwittingly, given encouragement to a reactionary school of theologians, will rejoice most at the official recognition of the value of his work. For, in former times, no one did more than Prestwich to lead geology out of bondage, and to point out possible lines of march through the wilderness. But for his far-seeing guidance in the past, we should not now be able to look together so far forward into the unexplored country that lies before us, and to discern there the streaks of light in which Sir Joseph Prestwich sees mists upon the hill tops, but in which his followers see the glow of dawn.

New Literature on the Foraminifera.

Foremost among the numerous works lately published on the Foraminifera is Professor Rupert Jones's "Monograph of the Foraminifera of the Crag." It is the second part of a paper begun in

1866 and published by the Palæontographical Society. This portion is practically a new work, as the long interval of time that has elapsed since part i. was issued has allowed the accumulation of much material, which has to be taken in zoological order. The paper opens with a valuable analysis of the stratigraphy of the Crags, written by H. W. Burrows, and dealing with all the beds from the Bridlington to the Lenham deposits. Those of the Coralline Crag are arranged according to Prestwich's zones, and for the first time are shown the characters of these divisions from their included Foraminifera. The descriptive portion of the paper opens with a short but lucid general account of the Miliolinæ (more elaborately discussed in Professor Jones's paper in *Ann. Mag. Nat. Hist.*; December, 1894), and is concluded by the descriptions of the species. In this connection we may mention the synonymies appended to each species, the result of considerable research, and limited mainly to those references not included by Brady in his great monograph in the "Challenger" Reports. A distinctly new and most useful feature is the section-plan of the various *Spiroloculinæ* and *Cornuspiræ*, a series of diagrams which greatly assist the worker in the comprehension of the different types. Three plates accompany the paper, which is a most important contribution to the literature of the Crag.

Among other recent publications devoted to this group of animals mention must be made of R. M. Bagg's "Cretaceous Foraminifera of New Jersey" (*Johns Hopkins Univ. Circular*; October, 1895), where ninety-four species are listed and carefully compared with their European representatives. Dr. Bagg has, we understand, sought the knowledge of some of his English colleagues, and has thus been enabled to publish a valuable and accurate list of forms, and one not burdened with the customary useless quantity of "new species," an advantage to science sufficiently rare to be worthy of high recognition. It is only to be wished that he had chosen a permanent medium of publication. Fornasini, still busy with his subject, has produced a report on "Foraminiferi della Marna del Vaticano illustrati da O. G. Costa" (*Palæont. Italica*, i.), in which many of Costa's forms are now correctly described for the first time. He has also published another of his privately-printed notes on *Lagena clavata*, d'Orb. var. nov. *exilis*. We should like to see these notes printed in the publications of some society, as they are inaccessible to the majority of students. Gustave Dollfus has given, in the *Annuaire Géologique* (1895), a careful digest of the literature of 1893, a year in which work on the group was particularly abundant. Frederick Chapman's paper on "Rhaetic Foraminifera" from Wedmore, in Somerset, has appeared in the *Annals and Magazine of Natural History* for October, 1895. This paper has been worked out with the author's customary care. It is the first exact account of Foraminifera from this horizon, and will be hailed with satisfaction by every student of the group. The most interesting part of the paper deals with Brady's genus *Stacheia*, which

was founded on obscure and imperfectly-known material of Carboniferous age. Chapman, as the result of his researches, has been enabled to complete and amend Brady's determination and diagnosis, and has thus avoided the objectionable method of founding a new genus for what are obviously similar forms, a result for which all zoological students are thankful. He has also been enabled to understand the obscure fossils known as *Psammosiphon*, Vine, as well as the "Plaques des Rayonnés" and *Asterocanthion* of Terquem and Berthelin, a result not less important than the elucidation of Brady's genus. Two plates accompany the paper. Dr. Rhumbler discusses the phylogeny of the entosolenian *Lagenæ* in the *Zoologischer Anzeiger*, no. 474, and the natural system of the Thalamophora in the *Nachrichten der k. Gesell. Wiss. Göttingen* (1895). In the latter paper he makes numerous new genera, but we have not the space to discuss their value here.

Australian Exploration.

From South Australia comes the Report of the Government Geologist, H. Y. L. Brown, on the Explorations of the Northern Territory of that colony, made by him between July, 1894, and May, 1895. The Report is illustrated by photographic views and geological maps and sections of much interest to students of Australian geology. The discovery of Carboniferous and Cretaceous rocks, identified by their fossils, which have been examined by R. Etheridge, junior, adds two formations to those previously known from this region. The Carboniferous rocks are on the coast, not far from Port Darwin, and Mr. Brown holds out the hope that they may be found to contain workable seams of coal. Auriferous rocks also have been discovered near the mouth of the Fitzmaurice River, and more may well be found in this hitherto unexplored country. Of even greater interest to geologists is the announcement that an undoubted *Olenellus*, of Scandinavian rather than American type, was collected at Alexandria, on the boundary between South Australia and Queensland, in the far north. The fossil is well preserved in a light yellow, slightly micaceous schist. Cambrian rocks containing *Olenellus* are already known in the Yorke Peninsula and other localities within 300 miles of Adelaide, also from the Kimberley district of West Australia. The present discovery considerably extends their range.

In our June number, vol. vi., p. 364, we gave an account of the investigations into sources of artesian water in Queensland. The Government Geologist, R. L. Jack, has now sent us his Report for 1894, which contains a detailed account of the explorations made in connection with this subject, together with a geological sketch-map of part of the eastern margin of the artesian water district of Queensland by himself and A. Gibb Maitland. Apart from the main question of water-supply, with which we have already dealt, the Report contains a few items of special interest.

Opportunity was taken to examine the "bone drifts" of King's

Creek, a locality from which a great number of the bones of marsupials and other extinct animals of Tertiary or Post-Tertiary age have been derived. Although time did not permit of a systematic search for bones, a collection was made of the shells of fresh-water Mollusca associated with them, of which little notice has hitherto been taken. These molluscs do not furnish many species, although individuals are incredibly numerous; all the species, with one doubtful exception, a *Unio*, are still extant in the same geographical area, *e.g.*, *Corbicula nepeanensis*, *Melania balonnensis*, and *Hadra jarvisiensis*. The question of age cannot, however, be settled until a larger collection has been made.

Upon certain grits and sandstones of Lower Cretaceous age there occur at certain localities, such as Flinders and Whetstone Station, curious markings, which are generally considered to have been made by aboriginals in grinding their tomahawks. The markings occur generally in groups up to five in number, and are individually up to a foot in length. They diverge slightly at one end, and are gathered into a bunch at the other. Mr. Jack points out certain difficulties in accepting the usual explanation, and suggests that the markings are the footprints "left on a beach sand by the passage of some heavy five-toed animal and partly filled up by the falling in of the sand."

In the valley of the Claude River, there occurs a formation consisting of grey shale, with which are intercalated beds of hard grey calcareous sandstone and grey sandy limestone. The shales contain numerous nodules of limestone, roughly cylindrical like segments of the trunks of trees, but with rounded ends; no organic nucleus has been discovered in these nodules. The sandstones and more sandy beds of the shales are full of large segments of silicified trunks of trees, in one of which were counted 130 rings of growth. The whole formation is absolutely unlike any other yet known in Queensland, and, although the country where it occurs resembles the "Rolling Downs," the age of the formation is possibly Permo-Carboniferous.

There are many other points of scientific importance which it is refreshing to find in a report that is professedly of a practical nature. The report by William H. Rands, on the "Leichhardt Gold Field and other Mining Centres in the Cloncurry District," which has been sent to us from the Survey, appeals more purely to the working miner. It appears that this area is likely to yield rich results if those who prospect there can overcome the difficulties of want of water and want of food. The former difficulty, at all events, will best be grappled with if the colony will extend a generous support to its energetic Geological Survey.

The Origin of Slugs.

Lieut.-Col. H. H. Godwin-Austen and Mr. W. E. Collinge have lately contributed a paper to the *Proceedings of the Zoological*

Society (1895, pp. 241–50; pls. xi.–xiv.) on some of those slug-like molluscs which, after the manner of our own little *Vitrina*, are too big for their shells. The specimens in question were collected in Borneo by Mr. Everett, who has added so largely to our knowledge of the Mollusca inhabiting that interesting region. The conclusions to which the authors came are, as might almost be expected, that these slug-like Bornean forms bear the same close relationship to the shell-bearing molluscs, among which they are now found living, as do similar forms in other quarters of the globe, and that future research will clearly show that many of the slugs cannot rightly be placed in families by themselves, but will find their true position before or after the genera they have developed into or are descended from.

THE APTYCHUS.

So much has been written on the structure and functions of the curious body or bodies known as the Aptychus, which are shaped something like the two valves of a *Trigonia* shell, and are found in connection with ammonites, that a paper recently published by Dr. Richard Michael, in the *Zeitschrift der Deutschen geologischen Gesellschaft* (*Jahrg.*, 1894, pp. 697–702, plate liv.; 1895) is of general interest. Dr. Michael has discovered in the museum of Breslau University a slab of Solenhofen slate with a specimen of *Oppelia steraspis*, the body-chamber of which is not merely closed by an aptychus, but contains some sixty tiny shells of young individuals, each with its own little aptychus. The conclusions drawn from this discovery are that the young of the ammonites were carried for some time in the shell of the mother, just as they are in the nidamental shell of the modern *Argonauta*, that the aptychus and the shell were both developed at a very early stage, that an aptychus was possessed by all the individuals of a brood, and therefore that the aptychus cannot be considered as a structure confined to the female and intended to protect the nidamental glands. As to the function of the aptychus, Dr. Michael endorses the opinion now generally held, that it was a true operculum to the shell, covering the mouth of the body-chamber when the animal was withdrawn into it.

THE EMBRYOLOGY OF CIRRIPEDES AND ISOPODS.

IN the last few years a large number of important memoirs have been published, containing the results of embryological investigations upon Cirripedia and Isopoda. Attention may be drawn to those by Theodore T. Groom (*Phil. Trans. Roy. Soc.*, London, vol. clxxxv., pp. 119–232); by Carl Chun (*Bibliotheca Zoologica*, heft xix., pp. 77–106; Stuttgart, 1895); by Carl Aurivillius (*Kongl. Svenska Vetenskaps Akademiens Handlingar*, vol. xxvi., no. 7, pp. 1–89); and by J. P. McMurrich (*Journal of Morphology*, vol. xi., pp. 63–154).

Of Mr. Groom's conclusions one of the most striking is this, that probably throughout the Cirripedia thoracica the structure of the

appendages in the Nauplius is almost exactly the same for all species. In the various forms examined he found the same number of joints on corresponding branches or basal pieces, and the same number of bristles, spines, or teeth on each of the joints, the agreement extending even to the distinction between simple and plumose spines and bristles. That earlier descriptions did not entirely confirm this conclusion was the less surprising, since sometimes Mr. Groom's own failed to do so; but he had only to see a difference in two of his sketches, "to discover upon re-examination of the object that a mistake had been made in one or other." From this perfect similarity he infers "that the character of the appendages is a primitive one, actually possessed by the common ancestor of the thoracica at some stage in its life-history." But this common ancestor is of ancient lineage, for "a careful study of the thoracic Cirripedes indicates that the Balanids, on the one hand, have probably diverged from *Pollicipes*, and the majority of the Lepads, on the other, from *Scalpellum*," these two genera being connected, as Darwin and Hoek (and more recently Aurivillius) have shown, by various intermediate forms. Hence it may well be said that the "permanence of such minute characters as the arrangement of the bristles on the appendages for the vast time represented by the Tertiary, Cretaceous, and probably, at least, part of the Jurassic periods, is highly remarkable, and well shows the slow rate of evolution which may take place in so highly specialised a group."

It is perplexing, however, to find that Professor Chun, though expressing the highest appreciation of Mr. Groom's work, by no means corroborates his opinion as to any precise agreement in the number and armature of the joints of the appendages in different Nauplii. There is little room for supposing that Professor Chun has given a wrong account of the facts under his own observation, so that, unless the appendages vary at different stages of nauplian life, it is likely that Mr. Groom's rule may have a less extensive application than he was led to expect.

Before leaving the subject of embryonic agreement, it will be worth while to quote the following passage from Dr. McMurrich :—

"The regularity of the entire process of growth of the metanaupliar region of the Isopods is most remarkable, and the more one studies it the greater is the wonder it excites. The regular rows of ectoderm and mesoderm cells are wonderful in themselves, and when there is added a more or less definite number of rows for all the species, we see that we are dealing with laws of growth which are at present far beyond our powers of explanation. It is true that the number of ectodermal teloblasts is not always quite constant, though approximately so, but it is exceedingly interesting to find that where, as in *Asellus*, they can be traced back to their earliest differentiation, there is a definite number of them—namely, eleven. And this definiteness of number is not confined to the Isopods, but is found also in *Mysis* (Bergh, 1893). As regards the mesoblast, however, the number is more constant, eight, and eight only, occurring in *Cymothoa*,

Ligia, and *Porcellio*, and again we find exactly the same number in *Mysis*."

Side by side with the more or less exact similarity of the appendages, Mr. Groom shows that there are numerous differences in other parts which distinguish the various cirripede Nauplii, though not in all cases very conspicuously, for those of *Conchoderma virgata* and *Lepas anatifera* can only be known apart by the slight fork in the tail of the latter. Of these two species the ova and embryos are almost indistinguishable. As a rule, however, differences between species go back as far as the new-laid ovum, affecting its size, shape, colour, and constitution. In *Balanus* the ova are brown, in *Chthamalus* orange-coloured, in *Dichelaspis* vermilion-red, in *Lepas* and *Conchoderma* blue. But the observations only refer to a limited number of species, and in *Lepas anatifera*, for example, "the blue colour becomes in older embryos replaced by red."

According to Mr. Groom, when the cirripede egg has been laid, a small mass of clear protoplasm is constricted off from it, known as the *first polar body*, the external hyaline layer of protoplasm meanwhile secreting a delicate firm pellicle—the vitelline or perivitelline membrane. Then, it is supposed, fertilisation takes place, the ova without this undergoing no further changes. But "the entrance of the spermatozoön has never been witnessed in Cirripedes." After fertilisation, the contents of the egg contract considerably, the protoplasm undergoes marked rhythmical contractions, the clear area at the anterior end becomes amœboid, and throws out short, blunt pseudopodia, which are often retracted. Then the *second polar body* is constricted off.

Here it may be noticed that in the isopods Dr. McMurrich places fertilisation before, instead of after, the formation of the vitelline membrane, for which, he says, "the *primary* condition is understood more especially through the observations of Fol and the Hertwigs (1887) to be normally a stimulus imparted to the egg protoplasm by the spermatozoön."

To return to the cirripede ovum. By degrees the protoplasm is mainly collected at the anterior pole and the yolk at the other. The yolk at this stage, Mr. Groom says, is devoid of a special nucleus, and, contrary to the general opinion, "is in no way comparable to an endoderm cell." He rejects Nussbaum's theory of a rotation of the plane which separates the protoplasm from the yolk. "The shape of the protoplasmic portion of the egg at this stage is generally ovoid, a central plug of greater or less extent reaching into the middle of the yolk, which thus fits like a thick-bottomed bowl on to the central mass." Another point on which Mr. Groom insists is that "the protoplasm of the first blastomere gives rise to a portion only of the ectoderm," and that "the second blastomere does not come from the first, but *from the yolk*."

When he comes to discuss the formation of the Nauplius seg-

ments, Mr. Groom remarks that most authors have transposed the dorsal and ventral surfaces, and that the whole account of embryonic development has been thus thrown into great confusion. The reason he assigns for the mistake is that the appendages appear first on the dorsal side of the embryo. "It is only, however, the free ends which are thus seen, the main part of the appendage being applied to the sides of the body, and the origin, as usual among appendiculate animals, ventral." It appears almost certain, he adds, that in copepods, as well as in cirripedes, "the surface of the embryo, on which the median longitudinal and transverse furrows appear, and which has been described as ventral, is in reality dorsal."

On the stimulus of light, Mr. Groom refers to experiments upon the Nauplii of *Balanus perforatus*, from which he and his colleague, Dr. Loeb, inferred "that light of sufficient intensity and duration ordinarily causes them to turn the oral lobe away from the light, while weaker light has after a time the contrary effect." He has probably not seen the criticism by Dr. Giesbrecht ("Pelagische Copepoden," p. 807), that the authors have misapprehended the distribution of light in a cylindrical glass filled with water, and illuminated from one side only, in which they have supposed that to be the darkest part which is really the most brightly lighted.

In a second paper (*Quart. Journ. Micr. Sci.*, vol. xxxvii., pp. 269–276) Mr. Groom passes to the latest nauplian stage, having had the opportunity of observing this metanauplian or immature Cypris-stage in a species of *Balanus* from Jersey. Professor Chun, on his part, has had the means of describing the metanauplian stage in two species of Lepadidæ. A little earlier Dr. Aurivillius published some remarkable facts as to the post-embryonal development in various species of *Scalpellum*. Here the metanauplian stage of the young was found within the capitulum of the mother, leading to the inference that, instead of half a dozen free nauplian stages, in these instances there is an abbreviated course of development, with none of the nauplian stages free.

In regard to the food of the Nauplii, Professor Chun testifies to finding in the intestines needles of Radiolaria, well-preserved skeletons of Dictyochæ, carapaces of diatoms, and remnants of Globigerinæ. He considers the bristles of the appendages well adapted for the ingathering and sieve-like retention of particles of food. Some of the long spines, armoured and glandular, are admitted to be weapons offensive and defensive. But on the whole he thinks that the rich apparatus of elongate spines and plumose bristles is chiefly subservient to flotation.

Two points in Dr. McMurrich's essay must be mentioned for the bearing they appear to have on the question now under debate of separating the tribe Chelifera from the Isopoda. Dr. McMurrich confirms the observation by Nussbaum (1891) that there is a transitory exopodite on the thoracic appendages in the embryo of *Ligia*, and

adds that the exopodites may also be seen, though less distinctly, in embryos of other isopods. Further, he finds in the development of *Jæra* a fold which extends backward to behind the first thoracic leg and represents a rudimentary carapace. Thus two of the characters which separate the Chelifera from the rest of the Isopoda apply only to a part of their life-history and lose at least some of their supposed importance.

In all the memoirs there are copious details worthy of discussion, but there is no space left even to trace the thrilling history of the "vitellophags," or to describe that movement of the nucleus which becomes at once so simple and so solemn when spoken of as karyokinesis.

THE SENSE OF ROTATION.

IN the second number of the "Princeton Contributions to Psychology," reprinted from the *Psychological Review*, under the editorship of Professor Mark Baldwin, is a paper by Mr. H. C. Warren on "Sensations of Rotation." Special arrangements were made to introduce, in addition to the internal sensations due to rotation on a turn-table, visual sensations. The subject was allowed to see, through an aperture in a screen at the foot of the rotating board, a number of strips of paper hung on two opposite walls of the room. Behind the aperture a mirror could be introduced, thus causing an apparent reversal of the direction in which the white strips seemed to be moving. It is well known that when a subject is rotating at a moderate and uniform velocity he supposes himself at rest. Only when the velocity is altered does he have sensations of being turned through a certain angle. If the uniform rotation is in any given direction an increase of velocity is interpreted as a slight turn in that direction, while a decrease of velocity is interpreted as a turn in the opposite direction. These results were confirmed by Mr. Warren's observations in a darkened room. But when the strips were illuminated and became visible there arose a conflict between the dicta of the internal sense and the dicta of the visual sense.

From a number of experiments, of which we cannot give the details, Mr. Warren draws the following conclusions: (1) that the internal sense of rotation is in the head alone and is something other than the general indication furnished by the vaso-motor system; (2) that the organ for the sense of rotation is the same as that for progressive movement; (3) that the results seem to favour the view that the semicircular canals constitute that organ. These conclusions differ from those reached on various grounds by Delage, Ayers, and others; but they seem to be drawn with caution from carefully conducted observations.

I.

Lamarck and Lyell: a short way with Lamarckians.

I AM much surprised to find my name in Romanes' list ("Post-Darwinian Questions," American edition, p. 14) of "the most prominent American Representatives" of the Neo-Lamarckian School, since I know of no reason for assigning this prominence to me except that I am an American. I trust, however, that, in order to define my position, I may be permitted to say that I studied the first edition of the "Origin of Species" with intense interest, and that I have been from that time an ardent disciple of Darwin, so far as his great law of selection is concerned; although I read at the same time the examination of the views of Lamarck in the "Principles of Geology," and was thus convinced that, while natural selection may possibly be no more than *a great but not the exclusive means* of adaptive modification, there is no evidence that the "Lamarckian principles" are among the other means for securing this result.

Since none of the modern Lamarckians seem to me to have answered Lyell's argument, I have seen no reason for changing my opinion. As this was formed years before the publication of Weismann's speculations, his *à priori* objection to the possible inheritance of "acquired characters" has not influenced me, since I have learned from Lyell that there is another question at issue—a question more fundamental and important than the question of their inheritance or non-inheritance.

In order to illustrate this I propose to examine Romanes' opinion that, if the inheritance of the influence of nurture is "in any degree operative at all, the great function of these (Lamarckian) principles must be that of supplying to natural selection these incipient stages of adaptive modification, in all cases where, but for this agency, there would be nothing of the kind to select." (Page 153, American edition.)

Unless these "factors" can be proved to have this "function" they are unworthy of consideration as a contribution to the history of adaptive modification; and I, for one, have found little to interest me in the interminable dispute as to the inheritance or non-inheritance of the effects of the conditions of life, because my study of Lyell taught me, long ago, that the gist of the whole matter is the deeper

question whether these effects are inherently adaptive. Lyell was ignorant of the principle of selection when he wrote the first edition of the "Principles"; but to us, who know the value of this adaptive mechanism, the question narrows down to the evidence that the "Lamarckian factors" can give rise to even "the incipient stages of adaptive modification."

As the strengthening of muscles by exercise is one of the simplest examples of the beneficial effect of the conditions of life, we may find instruction in its attentive consideration. In the strict sense of the words, it is not use, but increase in the food-supply, which enlarges the muscle, and this increase may be brought about by massage or by electrical stimulation as well as by exercise. Contractions and relaxations of the muscle increase the supply of food, because the muscle is so constructed that the nutritive fluids are drawn through it, in the right direction, by its normal contractions. The improvement of the muscle by exercise is the effect of a structural adjustment for securing this useful end—it is an adaptation; and the muscle is as obviously, if not as definitely, adapted for improvement by use as the heart is adapted for propelling blood. Exercise increases its efficiency only so far as structural adjustments for bringing this about already exist, and the real problem, the origin of the adaptation, is in no way different from that presented by any other structural adaptation.

This is still farther illustrated by the fact that organs are improved only by normal or natural use, while abnormal or unnatural use is well called abuse, as contrasted with use. It is only when our organs are used in that way which is popularly described as "the way they were intended to be used," that use is beneficial.

Romanes tells us, p. 59: "Inasmuch as we know to what a wonderful extent adaptive modifications are secured, during individual lifetime, by the direct action of the environment on the one hand, and by increased or diminished use of special organs and mental faculties on the other, it becomes obvious of what importance even a small measure of transmissibility on their part would be in furnishing to natural selection ready made variations in required directions, as distinguished from promiscuous variations in all directions. Contrariwise, if functionally produced adaptations and adaptations by the direct action of the environment are never transmitted in any degree . . . there would be an incalculable waste, so to speak, of adaptive modifications."

This argument has seemed, to many persons, a plausible one, but it is clear that it involves a most serious fallacy, unless the inheritance of the effect of the "Lamarckian principles" can be proved to be selective; for the ways to use a muscle are few, while the ways to abuse it are innumerable, and the inheritance of *all* the effects of the conditions of life must lead, not to "cumulative adaptation," but to cumulative destruction. Unless these factors can be shown to have, antecedently to selection, a determinate influence in beneficial lines,

it seems to be, on the whole, rather fortunate than otherwise that evidence of the inheritance of their effects is so hard to find.

Now is there any ground for the belief that the case is any different with intellectual and moral improvement? All known mental phenomena have structural correlatives, and are capable of development and improvement only so far as structural adjustments for bringing about this development and improvement already exist. Capacity for individual improvement under the normal conditions of life is an adaptation; the most wonderful and admirable of adaptations; and the beneficial influence of the "Lamarckian factors," so far as this influence is beneficial, is not an explanation, but a fact which itself calls for explanation.

Is there any evidence that the influence of the environment is inherently beneficial? If there is no such evidence we must believe that all its effects, except the effects which are already deducible from adaptive structure, must be hap-hazard.

When we remember how narrow the range of adjustment of each organism is, it must be clear that the probability that hap-hazard effects will be injurious or neutral rather than beneficial is prodigious. Even if they are inherited, the effects of the "Lamarckian principles" cannot cumulate in adaptation, except as an accident, which is so improbable that we are justified in doubting whether it has given rise to any specified adaptation until the possibility of a better explanation has been rigorously excluded.

While I find a difficulty in reconciling all Romanes' published opinions on this subject with each other, he seems to hold, p. 153, that "These Lamarckian principles are direct causes of determinate variation in adaptive lines," although it is clear that this must be proved before we can agree with him that "variation in these lines being cumulative, the result is that natural selection is in large part presented with the raw material of its manufacture—special material of the particular kind required, as distinguished from promiscuous material of all kinds."

Some fifteen years ago I published a book in which I sought to prove that we have in sexual reproduction a mechanism, produced by selection, for the purpose of causing changes in parts which need change, and that this mechanism expedites selection. My book found the oblivion which it no doubt deserved, but I suspect that it may have led, in some circuitous way, to the enrolment of my name in the list of Neo-Lamarckians, although I explicitly stated that I did not believe that these changes in parts which need change are adaptive; and I still believe, as I did then, that the "Lamarckian principles" must be proved to be adaptive, antecedently to selection, before we can seriously consider them as factors in adaptive modification.

There is, in Romanes' book, one sentence,—only one so far as I have discovered,—in which he seems to admit that this is not only

unproved but disproved. On page 73 he tells us, of the "Lamarckian factors," that "No question of value, as selective or otherwise, can obtain in their case." If we grant this, as I think we must; if we admit that these "factors" involve "no question of value," but that it is, to say the least, no better than an even chance whether their influence be good, bad, or indifferent, how do modifications produced by them differ from "fortuitous variations"?

Those who attribute the opinion that the so-called Lamarckian principles have not yet been proved to be factors in organic evolution, to the influence of Weismann, will do well to remember that we owe to Charles Lyell the demonstration that, until the influence of the conditions of life has been proved to be determinate, until their competency to cause a tendency to progressive development, antecedent to selection, has been made clear, they are of no value whatever as a contribution to the solution of the problems of adaptation.

It is quite possible that, in the long history of living things, adaptive structures have occasionally been produced by the fortuitous coincidence of fortuitous variations, but the chances against this are so overwhelming that we are justified in demanding demonstrative evidence before we accept this explanation of any adaptation. The production of words and sentences by turning a crank is not impossible, but many generations of readers have approved Swift's statement that this method of advancing knowledge failed to produce a single learned treatise.

Since all the past history of life is beyond our reach and cannot be made the subject of experiments, and since the scientific study of domesticated animals and cultivated plants is very modern, most of the evidence for natural selection is, and must be, indirect or deductive. Romanes holds, p. 57, that, since there seems to him to be the same sort of evidence of the influence of the "Lamarckian factors," these stand upon as good a logical footing, in the explanation of adaptive structures, as selection; but the cases are not parallel, and the sort of evidence which is adequate in the one case is totally inadequate in the other case.

If natural selection acts at all it must result in adaptation; while the advocates of the "Lamarckian factors" have yet to prove that these "factors" can account for any adaptive structure whatever, incipient or otherwise, except so far as this is the result of pre-existing adaptive machinery.

Satisfactory evidence that an event is the sequence of antecedents which are adequate may be totally unsatisfactory as evidence that it is the sequence of antecedents which seem inadequate. Indirect or deductive evidence may convince us that an adaptation is the result of selection, and may yet be totally unsatisfactory as proof that it is the result of the inheritance of the effect of nurture. Until this factor has been proved to be determinate and adaptive, the proof we must

demand before we can believe that it has produced adaptive modifications is like the proof which would convince us that words and sentences have fallen from a hopper filled with loose type. We must have demonstrative evidence that no undiscovered adaptive mechanism is at work.

Those who hold, with Romanes, that we find in nature structural adjustments which are inexplicable by selection alone, would do well to rest on their oars and look about them for some other determinate factor, or perhaps, for a better acquaintance with the resources, of selection, before they attribute them, without demonstrative evidence, to indeterminate "factors."

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II.

The Pigments of Animals.

Part I.

THE problems connected with the colours of animals have always attracted considerable attention, both on account of their intrinsic interest and, more recently, on account of their relation to general theories. It is, perhaps, an unfortunate consequence of this latter fact that the subject has been chiefly studied from the binomical standpoint. Important as this aspect of the question undoubtedly is, it has been of late so exclusively dwelt upon that it has been thought better in this paper rather to consider recent work from the standpoint of chemical physiology.

As is well known, the colours of animals are due to one of two causes, either to pigments deposited in the tissues, or to light-effects produced by the structure of the tissues. Of the meaning to the individual of the latter kind of colours we know practically nothing; but our knowledge of the pigments of animals is slowly but surely increasing.

We will take up, in the first case, the pigment hæmoglobin, not because of its importance in producing coloration, for this is rarely marked, but because, at least in the higher vertebrates, it has been carefully worked out from the physiological side.

First, as to the distribution of hæmoglobin. It occurs in the blood of the craniate vertebrates with the exception of *Amphioxus*[1] and the little fish *Leptocephalus*. In the striped muscles of vertebrates it is widely but irregularly distributed, often in a single species being invariably absent in some muscles, and invariably present in others. Of this peculiarity the rabbit is, perhaps, the most familiar example, but the common fowl affords another as well known to the physiologist as to the epicure. Other notable cases are those of the fish *Hippocampus*, where only the muscles of the dorsal fin are red, and of the rare fish *Luvarus*, where the difference between red and pale muscles is very well marked; but it would be easy to multiply examples almost indefinitely. Among the unstriped muscles of vertebrates, hæmoglobin is said to be found only in the wall of the rectum.

Among invertebrates, hæmoglobin shows the same peculiarities of distribution as in the muscles of the vertebrates. Thus it is present in the perivisceral fluid of some turbellarians, of *Glycera*, and of *Phoronis*; in the hæmolymph of *Lumbricus*, *Tubifex*, and other annelids; in the muscles of the pharynx in *Buccinum undatum*, *Littorina*, and other gasteropods; in the sheath of the nerve-cord in *Aphrodite aculeata*; in the cephalic slits of nemerteans, and so on.

[1] Its absence in *Amphioxus* has, however, been denied.

As to the function of hæmoglobin under the circumstances in which we know it best, that is, in the blood of vertebrates, there is, fortunately, no doubt. In this case, it is the active agent in the conveyance of oxygen from the organs of respiration to the tissues.

Among vertebrates, hæmoglobin has very rarely any part to play in producing colour. Such a function has been suggested for the human species, but the suggestion is quite unimportant for our present purpose, and hardly affects the general conclusion that in vertebrates the function of hæmoglobin is purely physiological. Of its origin and primary meaning in the metabolism of the animal we unfortunately know nothing ; on the question of its fate, however, we have at least the beginnings of knowledge. Leaving aside for a moment the question of the pigments derived from hæmoglobin, we may note generally that modern physiology teaches that hæmoglobin is only a stage in the metabolism of the animal. We now know that it is constantly being broken down, and as constantly re-formed ; that special organs—the liver, the spleen, the supra-renal capsules (?)—are concerned in these processes, and that the products of its decomposition are continually being expelled from the body. These facts certainly seem to suggest that the continued production of a large amount of hæmoglobin, which is actively employed in the physiological processes of the individual, involves physiological disturbances of great importance to the organism. That it is certainly a great strain upon the organism is confirmed by the great prevalence of anæmia among civilised races, whenever the conditions of development are at all unfavourable. That is to say, the organism under these conditions is unable to produce sufficient hæmoglobin to make up for that which is being continually decomposed. We thus see that while hæmoglobin is supremely useful in vertebrates, yet its presence is associated with modifications of the organism of fundamental importance.

So far, in dealing with hæmoglobin, we have had to do with certainties as regards function ; but the case is far otherwise when we come to the hæmoglobin of the muscles of vertebrates, and to the hæmoglobin of invertebrates. Here is no direct experimental evidence, but in spite of this there has been apparent a general tendency to assume that, wherever found, hæmoglobin has important respiratory functions, and to account for its irregular distribution in the muscles of vertebrates and in various parts of the body in invertebrates, on the hypothesis of special activity or special need. In point of fact, the proofs of universal usefulness which this hypothesis necessitates are difficult to obtain.

We will consider, in the first place, the question of the red muscles of vertebrates. As is well-known, the red and pale muscles differ in histological character and chemical composition ; and where the matter has been investigated, it has been found that the muscles differ also physiologically in their reaction to stimuli. Thus, under a single stimulus, pale muscles contract more rapidly and completely

than do red muscles, for the contraction of the latter is prolonged and sustained. This is in itself a curious fact, for the theory of increased oxidation in the red muscles would lead one to expect that such muscles would contract more rapidly and not more slowly than the pale. It may, of course, be said that the hæmoglobin provides a store of oxygen which is used up during the long-continuing contraction of the red muscles, but it is questionable whether the increased blood-supply to such muscles is not in itself sufficient for this purpose. In the rabbit, indeed, according to Meyer, the association between the colour and the other characters is not very close, some red muscles corresponding in character to the ordinary type of pale muscle. In this connection, we may note the well-known fact that insects possess two kinds of muscle, distinguished both by colour and by histological peculiarities; in this case, the extensive development of the tracheal system forbids the idea of the presence or absence of a special respiratory pigment.

Again, in fishes the red muscles are frequently those connected with the skin, and are certainly not remarkable for great activity. Krukenberg (7) suggests that in this case the hæmoglobin is of special importance in the respiration of the muscle, and is necessary on account of the superficial position of such muscles and the slowness of the circulation in fishes. In the fish *Luvarus*, however, he remarks that, besides the pale muscles and the red, there are muscles of an intermediate shade which he calls "halbroth." Are we to suppose that these muscles have only half as great a need of oxidation as those which are bright red? This question of the quantity of hæmoglobin is very important in the consideration of function.

As to hæmoglobin in invertebrates, we have the same hypothesis and the same difficulties. It is said that hæmoglobin is especially necessary to *Lumbricus* on account of its peculiar habitat; that its presence in the head-slits of nemerteans is essential for the oxidation of the brain; that it is present in the muscles of the buccal mass of *Littorina* because these muscles are especially active; and so on. On the other hand, many large marine worms have no hæmoglobin, whatever their habitat, and many gasteropods have none in their buccal muscles. Can we suppose that these muscles are less active in the limpet, the snail, and many others than in *Littorina*? Again, the amount of hæmoglobin present in the tissues of invertebrates seems to be usually small, so that the amount of oxygen which it can take up must likewise be small. Not that hæmoglobin in invertebrates has no function, but there is not much evidence to support the view that it is invariably of supreme importance to the organism in which it occurs. If this were so, we should surely find that hæmoglobin, when once acquired by the members of a group, would be retained by all their descendants, however widely they might diverge in other respects; and the irregular distribution of hæmoglobin among the invertebrate groups is contrary to this supposition.

We have already seen that in vertebrates hæmoglobin is constantly undergoing decomposition. As to the fate of the products of its retrogressive changes, there is reason to believe that the proteid constituent and the iron are re-employed in metabolism, while the pigment itself (hæmatin) undergoes various modifications. Thus the pigments of bile, bilirubin, biliverdin, etc., are almost certainly derived from the hæmoglobin of the blood; in *Amphioxus*, where hæmoglobin is absent, there are no bile-pigments (Hoppe-Seyler). Under ordinary circumstances, the bile-pigments seem to be reduced in the small intestine, and reappear in the fæces as hydrobilirubin (stercobilin). Urobilin and hæmatoporphyrin or iron-free hæmatin, other derivatives of hæmoglobin, are excreted in small quantities in the urine. Thus, so far as our present knowledge extends, the pigments derived from hæmoglobin do not under ordinary circumstances play an important part in producing coloration. The only notable exception is, perhaps, the pigment of the skin in the dark races of mankind, which is said to be derived from hæmoglobin. Under diseased conditions, on the other hand, the derivatives of hæmoglobin may produce marked pigmentation. Thus in jaundice the skin and conjunctivæ are stained with bile-pigment; in Addison's disease, or after removal of the supra-renal bodies, the skin is coloured with pigment which is said to be derived from hæmoglobin.

Again, although the derivatives of hæmoglobin are, in vertebrates, unimportant for the individual, they may have some importance for the species. Thus, according to Wickmann (17), the pigments of the eggs of birds are directly derived from the pigment of the blood which fills up the *corpus luteum*. In the *corpus luteum* the blood stagnates and undergoes retrogressive metamorphoses which result in the formation of derived pigments. This may be compared to a similar process in mammals, where hæmatoidin crystals are formed in the *corpus luteum*; the difference may, perhaps, be explained by remembering the difference in size of the eggs of birds and mammals, and the consequent diminished outflow of blood in the latter. The pigments of birds' eggs are thus related to bile-pigments in that both are derived from a similar source, *i.e.*, hæmoglobin. Wickmann further states that the pigments after their formation in the ovary are shed into the oviduct, in the uterine portion of which they are mingled with the materials of the shell. The differences in the pigments of different eggs may, he suggests, be due to differences in the composition of the blood itself. It is well known that in mammals the blood varies in different species, as is shown by the differences in the shape of the crystals of hæmatin, the colour of the plasma, and so on; similar differences may express themselves in birds as differences in the products of decomposition.

As to the occurrence of derivatives of hæmoglobin among invertebrates we have only a few isolated observations, mostly due to Dr. MacMunn (10), and largely dependent on the use of the spectroscope.

MacMunn claims to have found hæmatin in the tissues of many invertebrates, such as gasteropods, starfishes, and others. We unfortunately know almost nothing of the history of hæmoglobin in the invertebrates in which it occurs; but according to MacMunn the pigment of the skin in *Uraster rubens* is due to hæmatoporphyrin, probably derived from the hæmatin of the tissues. If pigments derived from hæmoglobin were found in invertebrates to any considerable extent, we should expect to find that they were less rapidly eliminated than in most vertebrates, and so played a more important part in coloration; but as to this there is no direct evidence.

As to hæmoglobin, then, its primary physiological significance is unknown. It is an unstable substance, by the retrogressive changes of which certain pigments are produced. Pigments, therefore, may arise as products of the katabolic change of a substance of direct importance in the physiology of the individual. In this case thay have no primary importance, but may acquire secondary importance as colouring agents, *e.g.*, in the case of birds' eggs.

Another method by which animal coloration may arise, is the direct introduction of pigment into the body by means of the food. This is less uncommon than might have been expected.

What might be called the simplest case is that of "green oysters," fully described some years ago by Professor Ray Lankester (9), and more recently studied by A. Chatin (1). As is well-known, the very marked pigmentation in this case is produced by a green pigment derived from a diatom taken in as food. The pigment is extremely stable, and apparently undergoes no change in the alimentary canal, and for some unknown reason is not eliminated with the excreta but absorbed into the tissues and there deposited. The great interest of this case in connection with theories of animal coloration is that no one has suggested that the colour of the green oyster is of any use to it. The coloration is a consequence of the nature of the food, and of the incapacity of the oyster to completely eliminate or to digest the pigment. Similarly. it is said that the colour of "red mullet" is due to a pigment derived from the crustaceans of its food. In his book on "Animal Coloration," Mr. Beddard gives some other instances.

As to this subject of extrinsic pigments, the most detailed information which we have is due to the observations of Mr. Poulton (11, 12), who has recently made some careful experiments on the subject. Mr. Poulton has chiefly worked at the colouring of caterpillars, and has found that in some cases the coloration is due to two sets of pigments: first, true or cuticular pigment, which is found in the cuticle; and secondly, adventitious pigment, found in the connective tissues, and forming a background to the other pigment. Some years ago, Mr. Poulton gave his reasons for supposing that this second kind of pigment was derived from the chlorophyll of the food; more recently he has endeavoured to prove this experimentally.

For the purpose of this experiment, Mr. Poulton obtained a

number of eggs of *Tryphæna pronuba*, and divided them into three sets. From the time of hatching, the three sets of larvæ were kept under similar conditions in the dark, and were fed respectively with the yellow etiolated leaves from the heart of a cabbage, the white midrib from which the yellow blade had been removed, and the ordinary green outer leaves of a cabbage. Of the three sets, the first and third developed well and showed almost identical colouring, the colour being in the early stages usually pale green, or occasionally dark green, but all turned brown, mostly dark brown, when maturity was reached. In this last respect they were contrasted with control specimens developed in the light, as these usually retained the green colour until maturity. This was apparently the only effect produced by the absence of light, which was necessary to avoid the risk of the conversion of etiolin into chlorophyll. In the second set, those fed with white midribs, the mortality was very great, only one larva out of a great number attaining complete maturity. The growth of these larvæ throughout was extremely slow, and they were invariably devoid of green colour, although the cuticular pigment was developed as completely as in normal larvæ.

Mr. Poulton's conclusion is that this experiment, taken in connection with previous observations on the nature of the pigment, tends to prove that the green or brown colouring matter in this species is derived from the pigments of the food, and may owe its origin either to etiolin or to chlorophyll. The experiment proves also that the cuticular pigment does not depend for its development upon the nature of the food. Mr. Poulton notes that the unhealthy appearance of the second set of larvæ may of course be held to indicate that it was owing to constitutional weakness that the larvæ were incapable of forming the green or brown pigment, and not to the lack of pigment in the food; but this is rendered improbable by the following considerations. The unhealthiness of the larvæ was most probably due to the fact that the white stalks upon which they were fed were so hard that it was difficult for them to obtain sufficient food, especially in the early stages when the mandibles were weak. (We might suggest that, in a repetition of the experiment, *chlorotic* leaves would be a fairer test.) Further, the single larva which did attain maturity was large and apparently in no way unhealthy. Again, the normal development of the true or cuticular pigment certainly suggests that the ordinary green pigment is in some way connected with the food. It is interesting to note that either etiolin or chlorophyll can produce the adventitious pigment of the larvæ. Both seem to undergo similar changes in the digestive tract, and in the altered condition find their way to the tissues. The whole of the pigment taken in with the food is apparently not retained within the body, for it was noticed that the fæces in the case of the first and third sets of larvæ were always yellow when fresh, but after exposure to the air oxidised and turned brown. This may be compared to the

change in colour of the larvæ themselves from green to brown, and is perhaps connected with a similar change of colour in solutions of chlorophyll when exposed to the action of organic acids or bright sunlight.

There seems little reason to doubt that Mr. Poulton's conclusion as to the green colouring matter in this case is quite correct, and that we have here a genuine example of the transference of pigment from one organism to another, with relatively slight change. As to the direct bearing on the physiology of the caterpillar, we have no detailed evidence; perhaps the following may not be thought to transgress the bounds of legitimate speculation. It is a well-known physiological fact that if one mammal be fed to excess with the fat of another, the fat which is ultimately deposited in the tissues of the former does not exhibit the specific peculiarities of the normal fat of the animal, but partakes more or less of the characters of the fat of the food. In other words, an animal is unable to impress its own individuality on the fat of its food, if this be ingested in very large quantity. Now both chlorophyll and etiolin undoubtedly contain a basis of lipochrome pigment, and the lipochromes are very frequently found associated with fats; in the case of chlorophyll, indeed, the association has been directly affirmed. Is it not possible that in the caterpillar—a notably voracious feeder—a process occurs similar to that noted above for mammals? That is, may not caterpillars, which have a practically unlimited food-supply, be unable to assimilate completely all the fat ingested, but yet have the power of storing-up in their tissues this extra fat, and with it the pigment with which it is associated in the food? If this be so, we can understand why in Mr. Poulton's experiments, when larvæ of set 2 were removed and furnished with etiolated leaves, they failed to develop green pigment, even when they continued to live for about a fortnight under the new conditions. These larvæ had previously been starved, and so were perhaps unable completely to assimilate all the constituents of the food finally supplied. According to this theory, food containing chlorophyll is a richer diet than food without it, and leads to the deposition of extra reserves in the tissues, and indirectly to additional pigmentation. The theory is not inconsistent with the fact observed by Mr. Poulton, that in some species (*Smerinthus ocellatus*, for example) the green colouring-matter may be found also in the eggs, and so be passed on to a second generation. As nutrition in the butterfly is unimportant, there is reason to believe that the caterpillar must provide the nutritive substance subsequently employed in the formation of yolk, and the association of lipochrome pigment with yolk is frequent enough; possibly the reserve substances deposited in the tissues may in such cases be directly employed in the production of yolk.

(*The References will be given at the end of Part II.*)

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III.

Foraminifera of the Chalk and of To-day.

WE have read with much interest the able and suggestive paper contributed by Dr. W. Fraser Hume to the October number of NATURAL SCIENCE, under the title, "Oceanic Deposits Ancient and Modern." We do not propose to deal directly with the main question underlying Mr. Hume's remarks, namely the conditions under which the Cretaceous deposits of Europe were laid down; but we venture to put forward some considerations upon the evidence adduced by him from the Foraminifera.

In the first place we observe that Dr. Hume, in dealing with an objection raised to the argument brought forward in his paper on the "Genesis of the Chalk,"[1] lays down as a general proposition that, "if a whole group of organisms retains precisely the same aspect throughout long ages, it may well be asked whether similarity of conditions during long periods should not be regarded as a determining factor in such a conservation of structure, and whether, therefore, the onus of proving objections to suggestions based on such identities should not lie with the objectors."[2] He does not, however, proceed to discuss the relationship subsisting between the Cretaceous Foraminifera as a whole and the corresponding fauna of recent seas; nor, indeed, does he deal with any considerable proportion of the forms found in the Chalk, but confines himself to a limited number of species, and those for the most part of the arenaceous type.

As to the general conditions under which arenaceous Foraminifera are found in recent seas, the writer appears to be seriously misinformed. He states[3]: "At the present day, the coarse arenaceous Foraminifera are found at depths rarely exceeding 400 fathoms." This, however, is by no means the case. To take the genus *Haplophragmium*: the "Challenger" Report[4] shows it to be plentiful at great depths, and at least four species have been found at a depth of 3,950 fathoms. We have ourselves recently had the opportunity, through the kindness of Admiral Wharton, F.R.S., Hydrographer to the Admiralty, of examining a large number of marine soundings,

[1] *Proc. Geol. Assoc.*, xiii., p. 221; May, 1894.

[2] NAT. SCI., vii., p. 270; Oct., 1895.

[3] NAT. SCI., vii., p. 273; Oct., 1895.

[4] Rep. Chall. Zool., vol. ix., pp. 301 *et sqq.* and p. 775; 1884.

including five from the Indian Ocean[1], taken at depths of 1,040 fathoms, 1,277 fathoms, 2,190 fathoms, 2,550 fathoms, and 2,694 fathoms respectively. From these five soundings we have obtained thirty-one species of arenaceous Foraminifera. Of these, only four are found in the first two soundings named, while thirty species occur in the remainder, the richest sounding being the deepest. The increase of arenaceous Foraminifera with depth is also strikingly shown in two soundings which we have recently examined from Torres Straits, about seventy miles east of Raine Island.[2] The first sounding, at a depth of 790 fathoms, yielded ten arenaceous species; the second, at a depth of 985 fathoms, yielded twenty species, including such coarse varieties as *Verneuilina propinqua*, *Reophax nodulosa*, *Haplophragmium latidorsatum*, and *H. globigeriniforme*.

In connection with the distribution of arenaceous Foraminifera, we may here remark that Messrs. Balkwill and Wright, in their paper on Recent Foraminifera from the Irish Sea,[3] have shown in a striking manner that arenaceous Foraminifera, while very common on muddy bottoms, are rarer on bottoms composed of mud with sand, and are absent from bottoms consisting of pure sand. Our own examination of the soundings already quoted also goes to show that the arenaceous Foraminifera flourish on bottoms which contain little or no admixture of free sand-grains.

We now proceed to consider in greater detail the evidence brought forward by Mr. Hume. He states: "Those species which are restricted to the Chalk Marl and Lower Grey Chalk are those which occur to-day at depths of less than 400 fathoms."[4] In support of this statement he cites *Textilaria turris*, *T. trochus*, *T. agglutinans*, *Gaudryina pupoides*, *Bulimina presli*, *Haplophragmium latidorsatum*, *Ammodiscus incertus*, *Tritaxia tricarinata*, *Spiroplecta annectens*, *Gaudryina rugosa*, and *Frondicularia archiaciana*.

1. Much stress is laid upon the occurrence of the three species of *Textilaria* together at the same spot, viz., off Culebra Island. We cannot, however, think that much importance should be attached to this. *T. agglutinans* occurs in all seas and at all depths, and probably few soundings could be taken without finding a specimen. *T. turris* and *T. trochus* are species very closely allied to each other, if, indeed, they are not mere varieties of the same form. *T. trochus*, the depressed and more common of the two, is plentiful in the shallow waters of many tropical and subtropical seas, and is by no means confined to localities having the peculiar position assigned by Mr.

[1] The exact localities are (*a*) lat. 6° 37′ 15″ N., long. 79° 26′ 21″ E.
(*b*) „ 6° 25′ 47″ N., „ 79° 24′ 14″ E.
(*c*) „ 2° 13′ 30″ S., „ 44° 13′ 00″ E.
(*d*) „ 2° 20′ 03″ S., „ 46° 3′ 06″ E.
(*e*) „ 2° 47′ 12″ S., „ 47° 39′ 45″ E.

[2] The exact localities are (*a*) lat. 11° 16′ 7″ S., long. 145° 14′ E.
(*b*) „ 11° 07′ 6″ S., „ 146° 39′ E.

[3] *Trans. Roy. Irish Acad.*, xxviii., pp. 317 *et sqq.*; 1885.

[4] NAT. SCI., vii., p. 274; Oct., 1895.

Hume to Culebra Island. For instance, we have ourselves found it abundantly in soundings from the Holothuria Banks off the north-west coast of Australia. It may further be mentioned that specimens have also been recorded from several zones of the Coralline Crag of the east of England.[1]

2. *Gaudryina pupoides*, again, is by no means characteristic of localities such as Culebra Island. The "Challenger" Report states it to be "a common deep-water foraminifer,"[2] and we have several specimens from our own Indian Ocean soundings at a depth greater than that recorded by Brady, namely, 2,550 fathoms.

3. *Bulimina presli* does not appear to be recorded by the "Challenger" Report from Culebra Island, but two forms usually looked upon as varieties of *B. presli*, namely, *B. buchiana* and *B. aculeata*, are recorded from that station.[3] The first-named, according to Brady, "affects tolerably deep water" and has a range down to 2,375 fathoms;[4] while *B. aculeata* is commonly found in deep water and has been recorded from a depth of 2,740 fathoms.[5]

4. *Haplophragmium latidorsatum*, as we have already mentioned, is a very common deep-water form, and has been found at a depth of 3,950 fathoms.

5. *Ammodiscus incertus*, as Mr. Hume admits, is "a marked feature in all modern deep-sea soundings."[6] We may add that its geological range extends to the Carboniferous strata, and it has been recorded from nearly all subsequent formations.

6. *Tritaxia tricarinata* can, we fear, afford evidence of very little value. It is extremely rare in recent seas. It was found by the "Challenger" at Raine Island only, and it is not recorded in Egger's "Gazelle" Memoir.[7] On the other hand, it was extremely abundant in Cretaceous times, and records of its occurrence in later formations appear to be wanting.

7. *Spiroplecta annectens* is recorded by the "Challenger" Report[8] from depths of 140 and 155 fathoms, both in the neighbourhood of Torres Straits. At the time the "Challenger" Report was written it had not been found elsewhere in recent seas; but we have lately obtained characteristic and well-developed specimens from the Arafura Sea at depths of 1,926 and 2,413 fathoms.[9]

8. *Gaudryina rugosa* is found in comparatively shallow water at the present day (11 to 675 fathoms), but it is met with at localities very different in character from Torres Straits.[10]

[1] "Foraminifera of the Crag. Part II.," p. 152. *Mon. Pal. Soc.*; 1895.

[2] Rep. Chall. Zool., ix., p. 378; 1884.

[3] *Tom. cit.*, p. 759.

[4] *Tom. cit.*, p. 407.

[5] *Tom. cit.*, p. 406.

[6] Nat. Sci., vii., p. 274; Oct., 1895.

[7] *Abhandl. k. bayer. Akad. Wiss., II. Classe*, xviii. (ii.), pp. 195–458; 1893.

[8] Rep. Chall. Zool., vol. ix., p. 376; 1884.

[9] The exact localities are (*a*) lat. 7° 23′ 8″ S.; long. 128° 48′ 18″ E.
(*b*) lat. 4° 28′ 56″ S.; long. 128° 2′ 12″ E

[10] Rep. Chall. Zool., ix., p. 381; 1884.

9. *Frondicularia archiaciana* can furnish, at the best, very unsatisfactory evidence. Its record as a recent form depends upon the occurrence of one specimen only (and that not quite typical) at Raine Island,[1] and there appears to be no other record of its occurrence since the Cretaceous era.

10. In reference to the question of the depth at which the Taplow Chalk was probably laid down, Mr. Hume places great stress upon the occurrence together at Culebra Island of *Verneuilina pygmæa*, *V. spinulosa*, and *V. triquetra*; and he states that "*nowhere else do they occur together*."[2] *V. pygmæa*, however, is, as stated by Brady, "a common deep-water foraminifer."[3] Its geographical range extends from lat. 60° N. to the Antarctic Ice Barrier, and its bathymetrical range from 129 to 3,125 fathoms. Its occurrence at Culebra Island can therefore prove little. *V. spinulosa* and *V. triquetra* are recorded together by Egger, in the "Gazelle" Memoir, from the neighbourhood of Mauritius, at a depth of 411 metres.[4]

We have thus dealt in detail with some of the forms cited by Mr. Hume. The process might be carried further; but we venture to think that enough has been stated to show that the evidence adduced by him has many weak points. We cannot refrain from again pointing out the unfortunate fact that, in bringing forward his evidence as furnished by the Foraminifera, he has altogether neglected many of the most characteristic of the Cretaceous forms, including nearly all those of the hyaline and porcellanous groups.

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[1] Rep. Chall. Zool. ix., p. 520; 1884.
[2] Nat. Sci., vii., p. 271, and *Proc. Geol. Assoc.*, xiii., p. 239; May, 1894.
[3] Rep. Chall. Zool., ix., p. 386; 1884.
[4] *Abhandl. k. bayer. Akad. Wiss., II. Classe*, xviii. (ii.), pp. 280, 281; 1893.

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IV.

Serum Therapeutics.

IT is a matter of common knowledge that an attack of scarlet fever or smallpox usually gives immunity from subsequent attack. This is true of the majority of specific fevers, but not of all: it will be within the memory of many that influenza confers no such privilege upon its victims, and the protection given by an attack of diphtheria seems to be of a very transitory nature. Of no disease is it absolutely true: second attacks even of smallpox are not unknown, while in measles and scarlet fever they are fairly common. It is also a familiar fact that individuals vary much in their susceptibility to infectious diseases; to most, children are notoriously more susceptible than adults, and the latter present wide individual variations in their liability to attack. The different races of mankind vary also in their susceptibility to certain infectious diseases; in some cases there seems to be a sort of natural racial immunity, in others, a relative tolerance seems to have been acquired by repeated exposure to a given infection for many generations, the most susceptible individuals having been weeded out. Natural selection plays its part, in fact, in our struggle against disease, as against other adverse circumstances; hence the mortality produced by certain of our common diseases when they are introduced for the first time among uncivilised races.

Similar facts are even more conspicuous in the susceptibility of the lower animals to infectious diseases. From some human diseases, such as typhoid fever and cholera, they appear exempt. Carnivora are remarkably insusceptible to tubercle and to anthrax; Algerian sheep are said to possess a racial immunity from anthrax; rodents, on the other hand, are exceedingly susceptible to most infections. In the case of nearly all pathogenic bacteria, it is the rule to find wide variations in the virulence which they manifest upon different species of animals.

Long before any explanation of the true nature of immunity was possible, attempts were made to produce it artificially in man against at least one disease—namely, smallpox. By the practice of inoculation, used for centuries in the East, and much employed in England in the last century, it was attempted, by selecting a mild and favourable case of smallpox from which to inoculate the healthy, to produce in them a correspondingly mild attack which should confer

subsequent protection; and the attempt was in a measure successful. The substitution of vaccination for inoculation was, at its inception, a purely empirical step, since at that time the identity of cowpox and smallpox was only a surmise. But in the light of recent knowledge vaccination receives its full scientific justification, as being a protective inoculation with a virus attenuated by transmission through a relatively insusceptible animal.

The discovery of the bacterial nature of infective diseases was a necessary preliminary to the right understanding of the true nature of immunity, and to Pasteur belongs the credit of first deliberately attenuating the virus of a specific disease with a view to protective inoculation, *i.e.*, to the production of a mild and non-fatal attack of the disease, which should confer subsequent immunity. Modern bacteriology has pressed chemistry into its service, and our knowledge of the mode of action of at least some pathogenic organisms in producing disease is tolerably clear.

Various explanations have been advanced of the essential nature of susceptibility and immunity. They fall into two groups, the chemical and the vital, and the truth probably lies between the two. To the vital group belong the doctrine of phagocytosis, advanced by Metschnikoff, according to which the leucocytes of the body are charged with the duty of destroying micro-organisms which gain access to it, and also Sternberg's theory of acquired tolerance to the toxic products of bacteria on the part of the cells of the body. It may be admitted that leucocytes possess the power of swallowing foreign bodies, such as bacteria, and they probably swallow them alive as well as dead; but Metschnikoff's attempt to represent the leucocytes of the body as a sort of local police, on the success or failure of which to cope with an invading army of micro-organisms immunity or susceptibility depends, is not a complete and satisfactory explanation of all the observed facts. Valid objections were soon urged, also, against some of the earlier chemical theories of immunity: such were the "exhaustion" theory, once advocated by Pasteur, according to which a given micro-organism, having once flourished in an animal body, used up some pabulum necessary to its growth and never subsequently renewed, and the "retention" theory of Chauveau, which postulated the retention in the animal body, as the result of bacterial growth there, of some substance produced by the bacteria, and inimical to their own further development. The view which has gradually gained ground as affording the best explanation of observed facts is the "antitoxin" theory, which is at once a chemical and a vital conception. It supposes that the animal body has the power of forming some substance either directly germicidal or capable of neutralising the toxic products of bacterial activity. The natural presence of such a substance gives immunity; its absence leads to susceptibility; its development, as the result of an attack of the disease, represents acquired immunity.

The reasons for accepting some such theory are shortly as follows. It has been known for some time that the freshly drawn blood of certain animals possesses germicidal power, and Buchner showed that such power resided in the serum. It is destroyed by heat. The body to which it is due appears to be a proteid, and in some cases, at least, a globulin: Buchner and also Hankin have isolated a germicidal globulin from the spleen and serum of rats. The germicidal power varies widely according to the animal from which the serum is derived, and is much more marked upon some species of micro-organism than upon others. Speaking broadly, the serum from an animal which is naturally immune against a given pathogenic organism possesses the greatest germicidal power for that organism, but there are exceptions to this rule. This germicidal power of animal blood is not the only one concerned. The action of pathogenic organisms in producing disease is mainly a chemical one. In many instances, such as tetanus and diphtheria, the bacilli remain localised at the seat of infection, and the severe constitutional effects of the disease are produced by the absorption of the virulent chemical poisons, or "toxins," to which they give rise. The existence of such toxins is not hypothetical: they have been recovered from the bodies of patients who have died, and they are readily produced in artificial cultures of the bacilli in question, from which they can be separated by filtration through unglazed porcelain. With the sterile filtrate, containing the toxins in solution, the phenomena of the disease can be in large measure reproduced in animals. The toxins have been found to be proteid bodies. Now it has been shown that, under certain circumstances, a substance is present in animal blood which can neutralise the toxins produced by pathogenic organisms even when it has not a direct germicidal action upon the organisms themselves. This, then, is a property of a different kind, but of equal importance: to this substance the term "antitoxin" should in strictness be applied. It has been found that serum from animals which have been artificially immunised against certain diseases by inoculating them with an attenuated virus is able to neutralise the toxins produced by the bacilli of those diseases, and also—a still more important step—that such serum injected into susceptible animals confers upon them immunity from the diseases in question. The first suggestions as to this important property were made in 1889 by Babès and Lepp in connection with rabies, but the credit of first clearly demonstrating the principles of serum therapeutics belongs to Behring and Kitasato, who in the following year applied it to tetanus and subsequently to diphtheria. They showed that serum from a rabbit which had been immunised against tetanus protected mice against virulent tetanus cultures, and further, that the toxins in a filtered culture of the tetanus bacillus were neutralised by admixture with the blood of an immunised animal. Similar facts have been shown for diphtheria, pneumonia, and some other diseases.

This body of evidence, which might be largely amplified, is sufficient to show the secure basis upon which the antitoxin theory rests. It is proved that germicidal proteids exist in the blood of some naturally immune animals: it is proved that, as the result of a non-fatal attack of an infectious disease, the subsequent immunity, of longer or shorter duration, is associated with the presence, in the blood, of a substance capable at least of neutralising the specific toxins of the disease in question, if not of killing the organisms which cause it. Klein has suggested that there may be both a chemical antidote, or true antitoxin, and a germicidal body in the blood of an immunised animal, and that one or the other, or both, may be present, according to the method of immunisation employed. It is not yet known how or where this antitoxin is formed—what tissue or organ is charged with its production, or whether it is derived from the very bacteria themselves. It seems likely that there may be a separate antitoxin for each specific infective disease: there is at least no evidence pointing away from such a supposition.

The discovery of antitoxic bodies in the blood is clearly one of the highest moment and capable of the most important practical applications. The success or failure of such practical applications will go far to prove or disprove the truth of the theories upon which they are based. The system of "serum therapeutics" is, however, the direct outcome of experimental facts rather than of preconceived theory. Though still in its infancy, enough has already been achieved in the case of diphtheria to warrant the belief that it is as valuable in practice as it is sound in theory.

The first practical step was the outcome of the observations of Behring and Kitasato on the immunisation of animals against tetanus. It was found that, in favourable cases, the symptoms of declared tetanus in animals might be removed or alleviated by the injection of antitetanic serum. Wider experience, however, soon showed that it was a much easier thing to prevent the appearance of symptoms than to cure them when they were once manifest. Nevertheless, in a disease so fatal and so intractable as tetanus, it seemed well worth while to try the effect of antitetanic serum in human cases. This has now been done in a number of instances and with varying success. Eight successful cases have been reported from Italy, treated with serum prepared by Tizzoni and Cattani; but most of them seem to have been slight or chronic cases, the natural mortality of which is not high. Roux and Vaillard have reported seven cases with only two recoveries, and the results of other observers have not been very encouraging. Hewlett has, however, collected 61 cases treated with tetanus antitoxin, with a fatality of 36 per cent., and this is a distinct improvement on the ordinary fatality of the disease, which, according to Conner, is as high as 80 per cent. in cases of wound infection; though less than 50 per cent. in cases of apparently spontaneous origin. Severe and acute cases seem to be less benefited by

the treatment than chronic cases. It is probable that, in a disease with an incubation period so long as that of tetanus (about ten days), the appearance of severe symptoms indicates that the mischief is done and that it is then too late to hope for much from serum therapeutics. It may be, however, that improved methods will overcome this difficulty.

Public attention has been chiefly directed to the application of the method to the treatment of diphtheria. Here success has been much more apparent, and a sufficient body of facts is forthcoming to allow of definite conclusions on the subject.

The original discoverer of the diphtheria bacillus was Klebs, but the demonstration of its causal relation to the disease is due to Löffler, while the work of Roux and Yersin has added important confirmation to the proof. It has been shown that the bacillus, remaining localised at the seat of infection, usually the throat, produces its constitutional effects by the powerful toxins to which it gives rise. With filtered cultures of the bacillus, containing these toxins in solution, it is possible to reproduce experimentally in animals many of the phenomena of the disease, including post-diphtheritic paralysis. Actual membranous inflammation of mucous membranes, such as characterises the disease in man, is with difficulty produced in animals by living cultures of the bacillus, though this can be done, notably in the trachea of kittens : nevertheless the organism is highly pathogenic for many of the lower animals, producing typically an intense local inflammation and swelling at the seat of inoculation, frequently with rapidly fatal results. Animals vary, however, in their susceptibility to the poison. The artificial immunisation of animals against diphtheria was carried out by Fränkel, Behring, and others, and Behring showed that the toxins in a diphtheria culture were neutralised by admixture with serum from an immunised animal, though the bacilli were not killed. Dogs and goats were found to be less susceptible to the disease, and hence to be more readily immunised than rabbits and guinea-pigs, but Roux's choice of the horse proved a happy one, since that animal is not only readily immunised, but on account of its bulk is capable of furnishing antitoxic serum in large quantities. The horse, therefore, is the animal now employed, and it can be immunised by successive inoculations, either with the filtered toxins from diphtheria cultures or with living bacilli. It would appear that the method advocated by Klein, in which, after a preliminary injection with diphtheria cultures sterilised by heat, increasingly virulent doses of living bacilli are successively inoculated, yields a good result in a shorter time than does the method of Roux, in which toxin only is injected, and, moreover, that the serum so obtained has more antitoxic power, and can therefore be employed in smaller doses than that of Roux. In any case, the effect of one dose must, as Behring pointed out, have completely passed away before the next is injected. After four or five inoculations, extending

over a period of several weeks, the horse is bled and the serum collected with all possible antiseptic precautions. By careful drying at a comparatively low temperature it can be stored safely without any loss of its antitoxic power.

Before use each sample of serum requires testing as to its antitoxic strength. This is done in the laboratory, and consists in determining what amount is required to protect a guinea-pig against a known standard fatal dose of a diphtheria culture, the result being stated in terms of the amount required to protect one gramme of guinea-pig. From this the dose for a child or adult human being can be calculated. With the more powerful antitoxic serums this amounts to about 10 c.c. With the weaker serums 30 c.c. or more have to be injected.

Before forming any opinion as to the results of antitoxin treatment in cases of human diphtheria, it is necessary to consider one or two important points as to diphtheria statistics. In the first place, diphtheria has not been in the past—and, perhaps, still is not—a disease to be recognised with certainty. It may be confounded with other exudative affections of the throat, and in its slighter forms is readily passed over as simple sore throat. According to Dr. Thorne Thorne, the main way in which it is spread is by means of slight and unrecognised cases in elementary schools. It is probable that we now have, in the presence or absence of the diphtheria bacillus, a positive criterion as to the nature of any given case, and no statistics are at the present time of much value unless based on bacteriological diagnosis. It follows that the statistics of past years are not wholly comparable with those of the present. It is to be noted also that, since only the more severe and well-marked cases are, as a rule, treated in hospitals, the fatality deduced from hospital statistics will be relatively too high. Further, the fatality of diphtheria varies very widely in different epidemics and in different years, so that statistics of one year are not necessarily comparable with those of another. The most reliable results will be attained by comparing the statistics of cases treated with and without antitoxin during a given epidemic. Since accurate bacteriological diagnosis is rarely practicable except in hospitals, hospital statistics are those that have the most value, and it is plain that the value of a new treatment, if it have any, will be most apparent among the severer cases which come into hospitals. Statistics of tracheotomy cases will have a special importance from this point of view, for these are the most severe and dangerous cases of diphtheria that have to be dealt with.

Diphtheria is a disease which has of late years shown a considerable increase in urban districts, though there is no evidence that its fatality has increased. In London, the statistics of the Metropolitan Asylums Board Hospitals show a fatality[1] which has fallen from 46·4 per cent. in 1888, when diphtheria was first admitted into

[1] *Fatality* means case-mortality.

these hospitals, to 29 per cent. in 1892, and rose again to 30·3 per cent. in 1893. This diminution is probably due, at least in part, to the fact that at first only a few cases were admitted, and those probably very severe ones with a high fatality. The fatality shows very wide variations, according to the age of the patients: the figures of these hospitals show that in children under five years old it is as high as 53·8 per cent., falling with each quinquennial period, till at the ages of fifteen to twenty it is as low as 3·9 per cent. Washbourne and others give the average hospital fatality in England as 38·9 per cent., and this is probably as accurate an estimate as is possible. The general fatality is, of course, much lower than this, as all the slighter cases which do not come under hospital treatment will be included. According to the figures of the New York Board of Health, the average fatality of diphtheria in that city for the four years preceding the introduction of antitoxin was 34·63 per cent. The average Berlin fatality is given by Baginsky as 41 per cent.; that of several children's hospitals in Vienna varies on either side of 50 per cent., which is about the same as that shown by the Paris hospitals for children.

When a new remedy such as diphtheria antitoxin is introduced, the temptation is to apply it indiscriminately to each and every case of the disease, however mild and favourable. Hence, in considering the statistics of diphtheria mortality under the antitoxin treatment, some allowance must be made for this fact: the figures may appear a little too good. There can, however, be no doubt that the fatality has been very materially reduced, in some cases to one-half or even less, and this is too striking to be attributable to mere variation in the epidemic constitution of the disease. We have not as yet a sufficient body of statistics from this country, but a few examples may be given. In December, 1894, Drs. Washbourne, Goodall, and Card reported 72 cases treated with antitoxin, all under 15 years of age, with a fatality of 19·4 per cent.: the previous fatality in similar cases had been 38·8 per cent. In February of last year, Dr. Ruffer reported 274 cases treated with antitoxin in four London hospitals, with a fatality of only 13·5 per cent. As regards tracheotomy cases, Washbourne, Goodall, and Card report 9, with only 3 deaths, the previous fatality having been 91·7 per cent., while Herringham reports 10 cases with only 3 deaths, the previous fatality having been 65 per cent. From the Belvedere hospital in Glasgow, Dr. Johnson reports 105 cases treated with antitoxin, with a fatality of 15·2 per cent. (excluding very mild cases and those who were moribund on admission): the lowest fatality in the previous five years had been 28·6 per cent., and the highest 41·4 per cent.

From abroad more extensive statistics are available. During the first nine months of the year 1895, the diphtheria fatality in New York has fallen to 19·43 per cent., with only partial antitoxin treatment: during the four preceding years it had averaged 34·63 per cent.

Roux, Martin, and Chaillou report from the Hôpital des Enfants Malades in Paris 300 cases treated with antitoxin, with a fatality of 26 per cent., the previous fatality having been 50 per cent. From Berlin, Dr. Baginsky reports 525 antitoxin cases with a fatality of 15·6 per cent., the previous fatality having been 41 per cent. Some of the most striking illustrations of the value of the antitoxin treatment are to be found in instances where the supply of serum has suddenly failed. At the St. Joseph Kinderspital in Vienna the average diphtheria fatality for ten years had been 51·1 per cent.: on the introduction of the serum treatment 27 cases were treated, with a fatality of 22·6 per cent. The supply of serum failed, and 32 cases had to be treated without antitoxin, and they showed a fatality of 65·6 per cent. When the supply was re-established the fatality in 21 cases fell to 19 per cent. Similarly at the Leopoldstädter Kinderspital in Vienna, the fatality, which had been 26·6 per cent. with antitoxin, rose at once to 66·7 per cent. when the supply of serum failed. The same happened to Baginsky in Berlin: his fatality rose from 15 per cent. to 51 per cent., when in August and September of 1894 the supply of serum failed.

Welch presents a summary of 82 reports on the antitoxin treatment of diphtheria up to July, 1895; the total number of cases, from all over the world, treated with antitoxin, amounts to 7,166, with an average fatality of 17·3 per cent., and these are nearly all hospital cases, and the great bulk from children's hospitals. He further tabulates 38 reports, containing 1,167 cases of laryngeal diphtheria, in which tracheotomy or intubation was required: the fatality in these under antitoxin treatment was 37·2 per cent., a very great improvement on previous figures, which showed in most instances a fatality of 50 to 80 per cent. in these cases.

It will be seen from the above statistics, which have been selected as giving a fair average of the results of serum therapeutics in diphtheria, that, in this disease at least, the method is passing out of the stage of trial into one of assured success. A reduction of fatality by something like 50 per cent. would mean the saving of not far short of a thousand lives yearly in London alone. And these would be young lives, since the improvement is shown to be mainly manifest in the earlier years of life. Nor is the statistical evidence the only evidence that can be adduced in favour of the new treatment; the general impression left on the minds of those who have employed it is strongly in its favour in nearly every instance, and the clinical impressions of physicians of large experience and known ability must be allowed great weight.

In the immunising power of antitoxic serum we have also a prophylactic of the highest value. It may be employed in the case of those children or other members of a household who have been exposed to infection. Dr. Hermann Biggs records from New York no less than four instances in which the prevalence of diphtheria in large

children's institutions was checked, and the disease completely stamped out by immunising the inmates *en bloc* with antitoxin. He states, however, that the immunity so conferred lasts only about a month, but this is in most cases all that is required.

The success which has attended the serum treatment of diphtheria has naturally led to attempts at preparing antitoxic serum for other diseases. Among these may be mentioned the "anti-streptococcus serum," which is designed to protect against cellulitis and erysipelas, and which already appears to have met with some success in practice. The elaborate researches of Tizzoni and Centanni on antirabic serum have still to bear their practical fruit, but there is good ground for believing that this will be the case. There appears, indeed, scope for the widest extension of the method. As fast as suitable animals can be immunised against a given infectious disease, so fast, we must believe, can their serum be employed as prophylactic or antidote in its treatment. The method constitutes an entirely new departure in medicine, and is indeed the greatest therapeutical discovery which this generation has witnessed. Every man of science will rejoice that at length experiment, in its rigid scientific sense, is taking its place as the foundation of a rational system of therapeutics in infectious disease.

F. W. Andrewes.

V.

Casual Thoughts on Museums.

Part III.[1]

Palæontological Museums.

PALÆONTOLOGY is an ugly word, which there was little need to coin. Instead of enlightening, it has largely mystified. In museums it has created mischief and confusion. As usually understood, it means the science of fossil creatures, which many people think are something quite different from living creatures, to be preserved in a different part of a museum, to be taken care of by a different set of men, and to be investigated by another class of students. Can anything be more preposterous?

Once upon a time, when men fancied that words like species and genus (really terms of logic, useful in the arrangement of our knowledge and nothing more) corresponded to actual facts in nature, and that every variation to which some ambitious student chose to give a name was a separate and independent creation, there may have been some excuse for a science of palæontology, or a museum or a department of palæontology, as there was for one of astrology or alchemy. Now that we know, or think we know, that living beings are linked to each other by long and continuous chains, perhaps including eventually all living forms and perhaps not, we must believe more or less in the continuity of life, and are nervously anxious to trace its pedigree. We have no patience with those who would separate our fathers and our grandfathers from ourselves because they happen to be dead, and refuse us the privilege of studying together the broken links which unite us to our primæval relations; if it be feasible to bring the links together for exhibition or for study, it ought to be done at all hazards. It is, in fact, impossible to separate them without absurdity. Where are we to draw the line?

A young American lady, who was looking through the museum at Lincoln the other day, asked my friend Canon Nelson what certain curious-looking stones were. He explained that they were fossils. "And what are fossils?" she asked. "The remains of animals and plants which lived a *very* long time ago, and are now preserved in

[1] For Parts I. and II., *see* Natural Science, vol. vii., pp. 97 and 319, August and November, 1895.

stone," said he. "Have you no fossils in America?" "Oh, no," she replied, "America is such a *new* country." There you have an object lesson in "palæontology."

Look at some of the results of this same reasoning. In the Natural History Museum there are some very interesting remains of the dodo, of which too many relics have not been preserved. It would be incredible, if it were not true, that the bones of this hapless bird have been fought over by two departments of the museum and have been torn asunder, and are now exhibited in two entirely different departments. A similar fight took place over the recently extinct Madagascar tortoise. The excuse for this absurd struggle is that these animals, although extinct, have so recently become so that they ought not to be called fossils. The fact is, there is really no gap at all between the graveyards in which we are now burying our fossilised friends and the graveyards of former days. There has been a continuity of deposition.

So much do I value this continuity, that I hold that in every specially geological museum, in which the different ages of the earth are successively illustrated, the last room should be always one devoted to the actual living things of to-day arranged according to their geographical distribution. The actual world as we know it is really the last chapter of the geological book.

If this be just, *à fortiori* must it be just and sensible that in a museum illustrating as nearly as may be the great lessons of *biology*, there should be shown, in sequence if possible, the various forms that bridge over gaps, and that people should learn early and often that these queer-looking bones and shells, etc., etc., which we dig out of the ground were really once parts of living animals more or less like those they now know, and were not mere sports and toys of Nature.

We are told that, however wise in theory, this is fantastic and unworkable in practice. I have been told so many a time, and I remain as obdurate as John Huss at the stake. In the first place, I claim to have on my side the two most accomplished and experienced directors and keepers of museums of my acquaintance, namely, the late John Gray and the present Sir William Flower; secondly, the plan has actually been tested in some cases, though not quite in the way in which I should like to see it carried out.

I must here point out that I am referring only to the exhibition part of the museum, and not to the study department; the latter is an entirely different matter. I am speaking only of what the great mass of philistines like myself, who ramble about the museum for pleasure and for profit, would like to see there. I hold very strongly that far too much is exhibited of certain things, and not half enough of others. Every type or rare specimen that will spoil or alter by exhibition should be removed from the cases and placed in the safest keeping possible. At the British Museum this has recently been done largely, to the joy of us all; but it is only a year or two ago

since such rarities as the Mauritius starling and the Labrador duck were exposed to the danger of ruthless destruction, while elsewhere in some of our great provincial museums, and in some of the great Continental ones too, such as Leyden and Paris, everything seems to be stuffed, and every animal, however rare and precious, is exposed to dust and sunlight and other causes of ruin and decay. It is really shameful, for in these matters we are trustees for those who come after, and if we are destroying the wild creatures wholesale, we ought at least to let our children see what they looked like.

These are not the only things that ought to be put by in lavender; everything that requires a special label to show that it is part of a fossil at all should be removed at once into the students' cabinets. The palæontological galleries at Kensington, which have been arranged with great skill and intelligence by Dr. Woodward, suffer greatly from this cause. They are literally crowded with broken and imperfect specimens, whose proper place is the students' room, where they are more accessible to the specialist and less distracting to the passer-by.

In the next place, it is a great mistake to exhibit too much. Do as we will, we cannot exhibit every species, we must make a selection; it is better, therefore, to exhibit a moderate number of properly mounted and labelled specimens, each of which teaches something, than to stuff the cases full of objects two or three deep, which nobody can see properly. Here again, referring to Dr. Woodward's galleries, I am bound to say that the ordinary philistine is quite bewildered by the immense number of duplicates exhibited. It is simply appalling to think of the herd of mammoths represented by some bone or tooth in the gallery of mammalia. It ought surely to be sufficient to exhibit as perfect a skeleton of the beast as we can get, as good a series as we can make to show his dentition at different ages, sections of the teeth to show their structure, and perhaps one series of disarticulated bones. It is merely a ridiculous mistake to exhibit vast rows of duplicates of the same thing, instead of remitting them to the study series. Geographical distribution should be taught by maps, not by reduplicated specimens, while a scale of strata can indicate geological distribution when necessary.

I hope these references will not be interpreted as meaning that I am not deeply sensible of, and deeply grateful for, what has been done for us all in this department of the museum by Dr. Woodward and his merry and aggressive boys since the removal of the collections from Bloomsbury. There is nothing like these fossil collections anywhere, either in their actual wealth, or in the way in which they are exhibited. Having said this, I cannot honestly, before passing on, avoid contrasting their work with that I have lately seen in another museum, namely, the Museum of the University of Oxford, where young men at an impressionable age are supposed to be inspired with the love of natural science, and a distaste for drinking and dissipation,

by what they see in their museum. I do not know, and I do not care, who is responsible for the state of things, but it is simply shocking to look over the cases devoted to palæontology. A more distressing show it is impossible to conceive: the specimens neglected and dirty, many of the type-specimens illustrating Phillip's Monograph of Oxford Geology unlabelled, the great collections from Kirkdale of our Father Anchises, Dr. Buckland, still in the hampers in which they were sent to the museum. How it is possible for an impressionable man to feel anything but loathing for fossils and their teaching after looking at this show, I do not know; and it is made more hideous when contrasted with the beautiful and most instructive preparations which are rapidly filling the cases in charge of Professor Ray Lankester and his assistants, to whom these neglected fossils might well be handed over.

Let us pass on, however! The burden of this homily is the exhibiting of the fossil and the recent specimens together in a biological museum. Now shells, whether of molluscs or brachiopods, are perhaps the most valuable of all things for showing the continuity that exists in nature; they run right through the geological record. In the case of large groups, like the ammonites, we can trace the intergradation of forms by the introduction of very small differences; we can see certain stocks represented at one horizon by a few kinds only, and in the next, perhaps, breaking into an afflatus of change, so that we get an immense number of species appearing, and then again as sudden a shrinkage. Lastly, we have the very interesting fact that certain genera such as *Lingula* have been persistent right through the long story of life. These, and other facts like these, are what arouse men's interest and invigorate men's ideas far more than looking at half-a-dozen cases full of beautiful shells. How are they to be illustrated unless we show our fossil forms and our recent forms together, instead of separating them and putting them at two remote poles of the museum? How is the student of fossil shells, especially of Tertiary and Post-Tertiary shells, to understand them or to classify them unless he is steeped to the finger-ends in a knowledge of the recent shells? And how is a man to understand the recent shells who never comes in contact with those ancestral forms, most of which are extinct, and which are so necessary to the proper interpretation of their descendants? Not only so, but it seems impossible to avoid creating fictitious species (which mislead us all in our induction, when following Lyell in discriminating the Tertiary beds by the proportion of extinct forms which they contain) if these Tertiary shells are discussed and described by men who have not a direct and intimate knowledge of the recent forms.

For these various reasons it seems to me as plain as can be that the fossil shells and the recent shells should be exhibited together and should be looked after and described by a common staff. This does not mean that the same man should have the same minute

knowledge of both sections. The British Museum is fortunate just now in commanding the services of two conchologists, remarkable not only for their knowledge, but what is more valuable than knowledge in a museum curator, namely, zeal and love for their work; and the galleries in which the Mollusca, both recent and fossil, are exhibited bear testimony to the work of Mr. E. A. Smith and Mr. R. B. Newton: nor should one overlook the valuable assistance rendered by Mr. G. C. Crick, Mr. B. B. Woodward, Mr. G. F. Harris, and Mr. H. W. Burrows. I am quite certain, however, that it would be a great gain if the two disjointed departments were united, and the shells arranged as they are at Caen, and I believe now at Paris, in a continuous series, not stratigraphically, but according to their affinities, with all the species of a genus placed together. Each genus should, so far as it is known, be represented by a model of the animal, with some illustrations also of its internal economy. If this were done, not only would it be a gain to us but to the curators themselves. I see no reason of any kind that is not fantastic against it. How slight a change would be required to effect this may be seen by any visitor to the gallery of Cephalopoda in the Geological Department of the Natural History Museum. Most of the recent genera are already exhibited here, either in the form of models, or drawings, or actual specimens. This gallery proves that the officials even of the Geological Department see the necessity for a large part of the reform here proposed, and the success that has so far attended their efforts is proof enough of the practicability of the scheme.

But the Mollusca and Brachiopoda are only one example out of many that might be brought to support the views of this paper.

HENRY H. HOWORTH.

SOME NEW BOOKS.

Faxon's "Albatross" Crustacea.

Reports on an Exploration off the West Coasts of Mexico, Central and South America, and off the Galapagos Islands, in charge of Alexander Agassiz, by the U.S. Fish Commission Steamer "Albatross," during 1891, Lieut.-Commander Z. L. Tanner, U.S.N., commanding. XV.—The Stalk-eyed Crustacea. By Walter Faxon. Pp. 292, with sixty-seven plates (ten of which are colored) and one chart. Memoirs of the Museum of Comparative Zoology at Harvard College, vol. xviii. Cambridge, U.S.A.; April, 1895.

The artistic plates by Mr. A. M. Westergren make this volume very attractive. His coloured drawings vividly suggest the beauty of deep-sea crustaceans as they appear when first brought to the surface. There is a certain splendour in the prevailing red tints, "which pass through various shades of pink, orange, and yellow, to straw color and ivory white." Some shore species are found to turn red when kept in the dark; hence Mr. Faxon is inclined to believe that in the deep-sea species the prevalence of red is "due to a modification of the pigments, induced by the darkness in which these creatures dwell, either through chemical action or more probably through a physiological process originating in the eye and affecting the pigment cells by a reflex action. In either case the prime cause is a purely physical one—the more or less complete absence of light in the depths of the sea." He proceeds to infer that "this color, then, is to be regarded as entirely useless to its possessor." Nevertheless, he presently contrasts the free-swimming Crustacea from great depths, which are "commonly of a very bright red color and endowed with visual organs of a high order," with the sedentary, concealment-loving, bottom species, which "are most often pale of hue and frequently blind." But surely, when difference of habit is thus associated with difference of colouring, it is illogical to take it for granted that natural selection is unconcerned in the coincidence.

To those who may wonder why deep-sea crustaceans should be red-tinted in general rather than of any other colour, Mr. Faxon gives Pouchet's explanation, that "the pigments of the xanthic series (red, orange, and yellow) in crustacea are contained in contractile anatomical elements—the chromatoblasts—while the blue pigment is never found in the substance of the chromatoblasts, but is held in free solution." Under the influence of the abyssal darkness there is supposed to be so great an expansion of the red chromatoblasts that any effect from the cyanic tints is completely overpowered.

In his Essay on the Distribution of Marine Crustacea, Mr. Faxon accepts the general opinion that the chief factor governing it is the temperature of the sea. He imputes inconsistency to Dana, who first of all divided the waters of the globe into five zones of marine life, "determined by isocrymal lines, or lines of equal mean temperature of the surface water during the coldest month of the year," and then proceeded to base his faunal areas, except in the two polar

zones, "chiefly on north and south lines running *across* the isocrymes, in accordance with the general trend of the great continental shores." So far, however, as crustaceans are concerned, the inconsistency is, perhaps, less the fault of Dana than of nature, which is for ever interfering with our methodical arrangements. It spoils in various ways the neat simplicity of zonal distribution. The isocrymal lines themselves are far from being exactly parallel. Beneath the warmest surface-waters there are temperate and frigid areas, of which species may, and sundry of them certainly do, avail themselves, to pass from zone to zone across the tropics. The commerce of mankind, as shown in Professor Catta's well-known paper, may help to distribute Crustacea in various directions, as well between north and south as between east and west. As Giesbrecht says in regard to the Copepoda, the arrangement of the fauna by latitude is much more favoured by the waters of the southern hemisphere than by those of the northern. Mr. Faxon seems surprised that Miers and Henderson in their "Challenger" Reports should have followed Dana's method. Obviously they did so simply because the facts relating to the Brachyura and to the Macrura anomala, so far as they knew them, agreed with Dana's plan. But Miers safeguards himself by observing that the researches of naturalists are always adding to the number of species common to the Indo-Pacific, Atlantic, and Occidental regions. Henderson declares that "the Atlantic and Indo-Pacific regions stand clearly apart from one another, each containing many species with a wide range of distribution, though, if the deep-water forms be excluded, a comparatively small number are common to both." Darwin, though unable to fit the cirripedes into Dana's scheme of distribution, nevertheless maintains on his own account that "the east and west coasts of the two Americas form two quite distinct cirripedial provinces" ("Balanidæ," p. 167). These considerations may be allowed to modify, without invalidating, the general proposition that the distribution of marine life accords with the zones of temperature. The arrangement may be compared to a textile fabric in which the warp is closely compact, while the transverse threads of the woof are comparatively few and scattered. This, indeed, seems to be Mr. Faxon's own view, although in his opening statement he disparages it.

By pointing out how large a number of littoral species of Crustacea in the so-called Panama province closely resemble species in the Caribbean province, Mr. Faxon makes an important contribution to the proof of the theory that, within moderately late geological times, there was water communication between the tropical Atlantic and Pacific.

A study of the deep-sea Crustacea, Mr. Faxon says, "leads to the conclusion that this fauna is one of cosmopolitan range, indivisible into subordinate local provinces like those of the littoral and terrestrial faunæ." Dr. John Murray has recently remarked that in the "Challenger" results there is "no striking evidence of a universal deep-sea fauna spread all over the floor of the ocean" (Summary, p. 1439), even if there may be an extended distribution of fishes and crustaceans which move freely over the bottom. Mr. Faxon's statement may be applicable to some orders or families of Crustacea, but it is far from having been proved as yet that it is applicable to all of them.

In the descriptive part of the volume it will be found that the fusion of the families Maiidæ and Inachidæ, proposed by Miss M. J. Rathbun, is accepted. In addition to this, there is more than one

suggestion offered for the fusion of genera, once more illustrating the point that the systematist is apt to find in nature, not a dearth of links and gradations, but an embarrassing glut of them. Among the many valuable specimens, new or rare, which are here carefully described, some of the most interesting are those belonging to the genus *Eryoneicus*, founded by Spence Bate on a single imperfect specimen half-an-inch long. As the "Albatross" obtained eight specimens, of which the largest was nearly two-and-a-half inches in length, various corrections and improvements in the description of these singular animals have become possible. It is discovered, for instance, that the ophthalmopods, instead of being absent, "consist of a large lobe, immovably fixed in a deep sinus in the anterior border of the carapace, this lobe sends forth an elongated cylindrical process outward and downward below the antero-lateral angle of the carapace; the anterior margin of the lobe, moreover, bears a prominent papilla, or tubercle." Among the Schizopoda of the deep-sea, Mr. Faxon describes one in which the eye-stalks are transformed to sharp spines, the visual elements being absent, and another in which also the eyes are absent, "their stalks assuming the form of slender styles whose tips are soft and delicate, perhaps serving as tactile organs."

In a work of this importance the characters of genera, revised and, where possible, condensed by so trustworthy an authority as Mr. Faxon, might appropriately have been given, and would have been highly welcome. What with the brevity of some authors and the prolixity of others, the student has no easy task in making out the real distinctions of genera, even with the requisite literature at his command. But how few have time or opportunity to supplement the study of Faxon by consulting the works of Roux and Rathbun, Leach and Lockington, de Haan and Dana, Stimpson and S. I. Smith, Miers and Alphonse Milne-Edwards, and the other eminent writers by whom these distinctions have been established.

In one small particular Mr. Faxon must be considered to have wilfully defaced his otherwise admirable volume. He takes upon himself repeatedly to alter the spelling of established names, with no prospect of any material advantage, and with the certainty of producing some amount of confusion. For example, the genera *Ethusa*, Roux, and *Ethusina*, S. I. Smith, are changed respectively into *Æthusa* and *Æthusina*. These altered forms are assigned to the original authors; but they are, in fact, new names, and, if accepted, would have to be accredited to Faxon. A change affecting an initial letter is particularly objectionable, because the name is thereby displaced in indices, and becomes difficult to find. But what is, above all, wanted in a scientific name is permanence. If every fastidious scholar is to be allowed to do a little furbishing, there can be no fixity. A change of fashion may insist that the Greek αἴθουσα should be transliterated neither into Ethusa nor Æthusa, but into Aithousa, or written in the character of the language to which it belongs. The latter change might have a chastening effect on the inventors of mongrel compounds. By improving Savigny's *Pasiphæa* into *Pasiphaëia*, Mr. Faxon inflicts on natural history a name of six syllables where one of four hitherto sufficed. As he himself points out, Risso had already in 1826 improved the name into *Pasiphae*. Risso's action was mischievous enough in using up a name which no less then three entomologists have since attempted to appropriate, but now we are brought to a pretty pass. If we cannot be content with the form which Savigny himself passed for press, namely, *Pasiphæa*, we must for the future range ourselves in opposing camps,

maintaining a ridiculous contest, not in natural history, but in mythology, to determine whether this genus of prawns ought rather to be named after the wife of Minos, or, by an Ovidian circumlocution, after her infamous daughter. The moral is that one name is as good as another—and a great deal better, if people would only leave off meddling.

After this emphatic discussion of a small blemish, it is pleasant to offer, for what it is worth, my earnest testimony to the high scientific merit and substantial excellence of Mr. Faxon's volume.

THOMAS R. R. STEBBING.

CONTRASTS FROM CAMBRIDGE.

PERIPATUS. By Adam Sedgwick. MYRIAPODS. By F. G. Sinclair (formerly F. G. Heathcote). INSECTS. (Part I.) By D. Sharp. Being vol. v. of the "CAMBRIDGE NATURAL HISTORY," edited by S. F. Harmer and A. E. Shipley. 8vo. Pp. xi. and 584, 371 figures in text, and a map. London: Macmillan, 1895. Price 17s. nett.

THE third volume of the "Cambridge Natural History," dealing with the Mollusca and Brachiopoda, has been followed at no long interval by the present volume—the middle one of three to be devoted to the Arthropoda. It is a natural course that each volume should be issued as it is ready; and no objection could be taken to the irregularity of the order, if the editors of the series were but careful to fulfil the promise of the prospectus that each volume should be complete in itself. But the promise has not been kept. The "Cambridge Natural History" is said to be intended in the first place for the instruction of persons without a knowledge of scientific language. What amount of instruction is likely to be conveyed by the two opening paragraphs (in which Mr. Sedgwick sets forth the reasons for classing *Peripatus* with the Arthropoda) to readers who are not told what an arthropod is? It is to be presumed that they must await the appearance of the fourth volume for information on this point. Meanwhile, they will doubtless joyfully agree that *Peripatus* is an arthropod when they read that it possesses "a vascular body-cavity and pericardium (hæmo-cœlic body-cavity)." Similarly, Mr. Sinclair's opening paragraph tells that the Myriapoda are "Tracheata with separated head and numerous segments." But for a definition of "Tracheata," the volume may be searched in vain.

It was inevitable that the section on *Peripatus* should have been entrusted to the naturalist whose brilliant researches on the structure and development of that animal have helped to spread the fame of the Cambridge science school. The twenty pages which Mr. Sedgwick has contributed, illustrated with well-chosen figures, mostly from his own memoirs, and with a map showing the distribution of the animals, will prove of value to the zoological student, but will hardly tend to enlighten the general reader. Many of the paragraphs are taken word for word from Mr. Sedgwick's monograph in the *Quarterly Journal of Microscopical Science*; and the style of writing appropriate to the pages of that magazine is out of place in a book intended to instruct the intelligent layman in the facts of zoology. Technical terms are constantly used without explanation. There are people who want to be told what a gastrula is. Tracheæ and nephridia are described, but not a word is said about the use of either set of organs. The editors of the series should surely make it their business to see that each contributor at least attempts to fulfil both the functions—work of reference and popular exposition—claimed for

the series. Mr. Sedgwick could not fail to succeed in the first; it is all the more to be regretted that the structural and developmental characters which make *Peripatus* a type of such high interest to the zoologist have not been set forth so as to appeal to the common man. The paragraph in which Mr. Sedgwick dwells on the external beauty of a living *Peripatus* with its "velvety skin," and "eyes sparkling like diamonds," shows that he could have told with equal plainness and enthusiasm, had he taken the trouble to do it, the internal history which he has so ably read.

To the account of *Peripatus* is appended a synopsis of the species. This can only be of use to the systematic naturalist. When this despised person studies it, he will marvel that three men of the scientific standing of Messrs. Shipley, Harmer, and Sedgwick could have sent forth such a piece of work. Not even a footnote calls attention to Mr. Pocock's opinion (published July, 1894) that the Neotropical, Ethiopian, and Australian sections of *Peripatus* are fully worthy of generic distinction. The names of all the authors of species are put in brackets; as if they had referred the forms they described to some other genus. Mr. W. L. Sclater is credited with a name (*P. demeraranus*) which Mr. Sedgwick himself needlessly coined to supersede Mr. Sclater's name for the Guiana species; while the name *P. chiliensis*, ascribed by Mr. Sedgwick to Gay, is another gratuitous synonym of his own for the species which Gay and Blanchard called *P. blainvillei*. Mr. Dendy's Victorian species, *P. insignis*, is not mentioned at all. *P. iuliformis*, Guilding, and the *Peripati* from Jamaica and Dominica are inserted among the "doubtful" species, though all three have been recently described in sufficient detail by Messrs. Grabham, Cockerell, Pollard, and Pocock. Lastly, *P. trinidadensis* is notified as a "n.sp."; it was described by Mr. Sedgwick himself in 1888. This last marvel gives the clue to the whole performance; the synopsis has been exactly reprinted from Mr. Sedgwick's monograph, now seven years old! To point out such slovenly compilation as this is condemnation enough.

Mr. Sinclair's chapter on the Myriapoda is a very uneven piece of work. It is written in a style well calculated to allure the reader to the study of those animals, often thought repulsive and uninteresting. In the introduction the author gives a general account of the aspect, habits, feeding, and breeding of the myriapods, incorporating many personal observations; he also discusses the popular names in use for the creatures. Then follows a section on the classification of myriapods, which can only be described as inexcusably antiquated, especially as Mr. Sinclair seems deliberately to have chosen to make it so. He implies that Koch and Latzel are the only writers who have treated of the myriapods as a whole; Messrs. Pocock and Bollman are presumably included among "the many authors who have done excellent work on different groups and families," work which Mr. Sinclair "does not wish for a moment to undervalue." Then why does he utterly ignore it, when both those writers—in English—have treated the group as a whole, and in a masterly way? But if Mr. Sinclair turns from Pocock and Bollman, surely he will consent to be guided by Latzel? No, "on the whole, he thinks it will be better to take the classification of Koch," though the works of that author are "comparatively old," one only thirty and the other fifty years. And so the reader is presented with a scheme the diagnostic statements of which bristle with errors. The genital ducts of millipedes are said to open on the seventh segment (instead of the third, as Mr. Sinclair correctly states in his anatomical section). The Callipodidæ and

the lately rediscovered *Glomeridesmus* are not mentioned at all. The Polydesmidæ are said to have always nineteen body-segments; most of the genera have twenty. The Glomeridæ and Sphærotheriidæ are credited with eleven and twelve body-segments respectively; in each case one too few. Not a word directs attention to the peculiar suctorial mouth of *Polyzonium*; and the openings "near the lateral corners of the body-rings" in that animal are not stigmata but stink-holes. Turning to the centipedes, we are told that the Lithobiidæ have "many eyes"; *Henicops* has but one ocellus on each side. Clawed anal legs are given as a family character of the Geophilidæ; in several *Geophili* these limbs have no claws. The dorsal plates of *Scutigera* are said to be smaller than the ventrals; they are always larger, as Mr. Sinclair must surely know from his own studies on the animal. Haase's interesting genus *Cermatobius* (forming a distinct family) is, of course, not mentioned at all. If Mr. Sinclair had taken the trouble to find out the existence of this intermediate form between the Lithobiidæ and *Scutigera*, he would perhaps have hesitated to raise the latter genus to ordinal rank; equal in importance to the Chilopoda, Diplopoda, Symphyla, and Pauropoda. But it is really astounding that a chapter on Myriapoda, in a work professing to give the latest results of research, should contain no reference to the view—several years old, and independently advocated by Pocock and Kingsley—that the association of these groups in a single class is unnatural, and that the Chilopoda and their allies are closer to the Insecta than to the Diplopoda. Suppose this chapter had been entrusted to a classifier, who, considering it necessary to put in something about development, might have written that, "without desiring to undervalue the work of several recent investigators, he thought it better to summarise Newport and Metschnikoff's accounts of myriapod embryology": such treatment of the subject would appear incomplete and one-sided to Mr. Sinclair, yet he has treated the systematic aspect of the group in a manner precisely analogous. It is, of course, a mere truism that embryological and systematic work are alike necessary for the progress of zoology, and that the workers in either branch should feel the unity of aim underlying their diverse labours; but such a chapter as this shows that there may still be need of exhortation to naturalists to take an intelligent interest in each other's subjects.

The classificatory section is followed by an account of the structure of myriapods. This is well and clearly written, and the author evidently remembers that his task is to instruct the unlearned as well as to guide the serious student. Indeed, the latter might complain that from his point of view some features are passed over too briefly. Even here errors are not wanting: all millipedes, except the Polyxenidæ, are said to possess stink-glands; the Chordeumidæ have none. More space might have been devoted to such very interesting and problematical forms as *Scolopendrella* and *Pauropus* and figures of these should certainly have been given. The illustrations of entire animals are all from Koch, and the *Scutigera* wants several pairs of legs, though no comment is made as to its imperfection. The structural figures are after Latzel, copied from Mr. Sinclair's own papers, or original.

The only fault that might be found with the section on the development of myriapods is that it is drawn too exclusively from Mr. Sinclair's own researches. It consequently neglects the centipedes; but it is an excellent summary of diplopod development, and serves admirably the twofold purpose claimed for the book. Mr. Sinclair shows how embryology can be made attractive to the general

reader, and how the fascinating problems presented by the facts of development can be set forth at once plainly and accurately. These pages are well illustrated from the author's own works. A section on fossil myriapods, with a few figures, is a welcome addition; but, here again, neglect of recent literature is to be deplored. Two important families of Carboniferous centipedes, the Eoscolopendridæ and Gerascutigeridæ, erected by Scudder in 1890, are not mentioned. The genus *Trichiiulus* still figures among the Archipolypoda, though the fossil on which it was founded is truly referable to a fern-frond, as stated by Zittel. Among the fossil diplopods, amber specimens of *Craspeodosoma* are referred to the Lysiopetalidæ, a family name not to be found in Mr. Sinclair's Kochian classification of recent forms, where the family to which *Craspeodosoma* belongs is called Chordeumidæ. In conclusion, Mr. Sinclair sums up the light thrown by embryology and palæontology on the origin of the myriapods. But, here again, we are faced by the author's strange apparent ignorance of the fact that naturalists of high repute deny the homogeneity of the class. The failing common to Messrs. Sedgwick and Sinclair in their contributions to the present volume is their unaccountable neglect of the work of all recent authors—except themselves.

The first instalment of Dr. Sharp's work on the Insecta occupies six-sevenths of the volume. High expectations must have been formed by British entomologists of the character of this work. They will not be disappointed; for Dr. Sharp has given the naturalists of to-day such a gift as Westwood gave their fathers in his "Modern Classification." Dr. Sharp cannot claim to have done original research on the structure and development of insects comparable to Mr. Sedgwick's work on *Peripatus*, or Mr. Sinclair's on the diplopods. He belongs to the despised group of the systematists; but his contribution to the volume, in the excellent balance of its subject-matter and the fulness of its references to the literature, contrasts pleasantly—and painfully—with the chapters written by the two eminent morphologists. A perfect work on modern entomology is beyond the power of man; but Dr. Sharp has set before himself a high ideal, and has gone far towards realising it. Opening with some remarks on the immense success achieved by insects in the struggle for life, and the high interest of the social organisation displayed by many of them, he proceeds to an account of their external and internal structure, embryology, and subsequent development, illustrated by numerous figures, many of which are original. All technical terms are explained, and any general reader who will take a little pains can hardly fail to grasp the the author's meaning. To the student, these pages are a veritable store of information containing a judicious summary of the most reliable observations of numberless workers. And if any particular problem calls for an appeal to the original authorities, nearly every page is provided with several references in footnotes. In the embryological section it might have been well to devote more space to the problems presented by the segmentation of the head. The antennæ are stated to arise from the procephalic lobes, though there is a general agreement among recent workers (to some of whom reference is made) that these organs arise primitively behind the mouth and are serial with the limbs.

Dr. Sharp's chapter on classification opens with the alarming statement that the 250,000 known species of insects probably form but a tenth part of the insect population of the globe. It is a surprise to find that Dr. Sharp, though summarising the recent general classifications of Packard and Brauer, holds to a scheme differing but

slightly from that of Linné. As he points out, the most serious difficulty in the arrangement of insects in orders is presented by the Linnean Neuroptera. This "order" he prefers, though provisionally, to keep intact, in spite of the vast difference in metamorphosis shown by the groups composing it. It cannot be denied that such conservatism is convenient, but it might have been well at least to have separated the mayflies from other "Neuroptera" (as well as the earwigs from the Orthoptera), the paired genital openings in these groups being surely an archaic character of high import. It is hard to accept Dr. Sharp's objection to the use of such characters on the ground that but few insects have been examined for them. Zoology can only proceed on the assumption that the correlation of certain external structures with certain internal structures will be found to hold throughout a natural group. A museum ornithologist will not hesitate to class a new skin with feathers and beak as an egg-laying bird, though it is but a presumption that the living creature did lay eggs. The peculiar structure of the mouth-organs in caddis-flies, also, seems to warrant ordinal rank for them. Dr. Sharp, though restoring the Linnean name of Aptera for the Collembola and Thysanura objects to Brauer's grand division of insects into winged and wingless. Many of the "winged" insects, he points out, have no wings. But surely the idea of the division is that in the one group the absence of wings is a primitive character—in the other secondary.

The orders of Insecta described in detail in this first part of the work are the Aptera, Orthoptera, Neuroptera, and Hymenoptera, as far as the Trigonalidæ. This arrangement is certainly open to criticism, for if any two orders of insects should be brought closely together, the Lepidoptera should follow the caddis-flies. And Dr. Sharp admits that the Hymenoptera have good claim to stand at the head of the insect-world—therefore to come last, if the Aptera begin the series.

But the way in which Dr. Sharp treats the details of his subject is, as in the introductory chapters, admirable. One is especially struck by the balance which is maintained all through between various branches. In the account of each family, external form, morphology, development, habits are all adequately dealt with and excellently illustrated; while the more solid matter is diversified by discussions on such points as the power of the book-louse *Atropos* to produce "death-tickings," and the hypocritical devotions of the Mantids. On every subject, while old authors are not neglected, references to the latest work are given. The anatomist will find a summary of Grassi's recent researches on the Thysanura; the lover of insect habits, the same naturalist's observations on the Termites; the systematist, descriptions of Hansen's *Hemimerus* and Simon's blind cockroaches from the Philippine caves; the lover of controversy, a notice of Spencer and Weismann on the sexes of ants; while foot-notes on every page will send off the industrious student to consult the original monographs.

In his treatment of families, Dr. Sharp is rather inclined to "lump." For him all dragon-flies are but one family, all caddis-flies but one. These views do not seem to be at present those of naturalists who have specially studied the groups. It is remarkable, however, that in many orders, after a period of splitting into numerous families, has come a time of reunion. This has been the case with moths, beetles, and bugs, and it is possible that Dr. Sharp's views of the true value of family groups in other orders to-day may become those of

the specialists in a few years' time. The old division of the Hymenoptera into terebrant and aculeate sub-orders is abandoned for the more natural division into groups with sessile and petiolate abdomens.

The remarks on fossil insects might have been somewhat fuller, and no figures are given of the primæval forms which are classed by Scudder as Palæodictyoptera. However, Dr. Sharp mentions the difficulties of interpreting these insect remains, as well as the views of Brogniart and others that they are better classed in distinct families distributed among certain of our existing orders. Altogether, Dr. Sharp has produced a book to which the British worker at entomology will, in the first place, turn for information; in which the intelligent reader will find justification for the deep saying of Augustine[1]—comparing "grubs" with "angels"—which has been chosen as the motto of the volume.

GEO. H. CARPENTER.

BURMESE SPIDERS.

DESCRIPTIVE CATALOGUE OF THE SPIDERS OF BURMA, based upon the collection made by Eugene W. Oates, and preserved in the British Museum. By T. Thorell. Pp. xxxvi., 406. London, 1895. Printed by order of the Trustees. Price 10s. 6d.

ANY publication on Arachnida from the pen of Professor T. Thorell will be hailed as a valuable contribution to arachnological science, especially as in the work now under notice the productions of a region like that of Burma are not only for the first time systematically collated, but the number of known species in the Order (Araneidea) treated upon increased from 175 to 381. Of Mr. Oates's collection, numbering 310 species, 153 are described as new to science and 206 new to Burma, 19 new genera being also characterised. The collection was chiefly made at Tharrawaddy, about 70 miles north of Rangoon, in what used to be called British Burma, but now, since the annexation of the whole country, is termed Lower Burma. With the exception of a short introduction, in which Dr. Thorell explains the classification he has followed in the present work, some remarks on zoological nomenclature, a reference to the literature already published on Burmese spiders, and a note on the general character of the Burmese spider-fauna, the volume before us consists simply of a series of scientific descriptions, drawn up in Latin and detailed with the author's well-known minuteness and accuracy; it is therefore pre-eminently a work for the working araneologist. It is maintained by some zoologists that illustrations are quite unnecessary where natural objects are fully and properly described; but, unquestionably, well-executed drawings are of incalculable assistance, and not only save the scientific worker much valuable time, but are a great attraction to others, and may often be the means of inducing those who may chance to have the opportunity, to turn their attention to the observation and collection of the objects delineated. It seems strange, therefore, that the Trustees of the British Museum, having the materials in their own hands, should have permitted a work like the present to be issued without a single illustration of any kind.

With respect to the character of the Burmese spider-fauna as at present known, out of the 381 species described, 106, or rather over

[1] "Creavit in cœlo Angelos, in terra vermiculos: non superior in illis, non inferior in istis. Sicut enim nulla manus Angelum ita nulla posset creare vermiculum."

one-third, belong to the group "Orbitelariæ" (*i.e.*, those spiders whose snares are of an orbicular or wheel-shape). Four other groups, the "Retitelariæ," "Citigradæ," "Laterigradæ," and "Saltigradæ," number 218 species, in about equal proportions; this at once indicates the nature of the collecting-work as yet effected in the region in question. The spiders of all the five groups named are those which obtrude themselves at every step upon the most casual observer. By far the greater number of species have to be searched for with diligence and the expenditure of much time and labour, and the results would probably be, in general, found to represent largely the groups so very scantily, as yet, recorded from Burma. Three hundred and eighty-one species, though a very respectable and valuable total, can scarcely be a fourth part of what Burma possesses and would probably yield to a careful and systematic search. It is to be hoped, therefore, that Mr. Oates will renew his researches, especially keeping in view those species that require to be searched for in the most uninviting places, and that we may have another volume upon the results from Dr. Thorell's pen, and well illustrated; under such conditions, the history of the Burmese spiders would be very complete and valuable—more so, perhaps, than that of any country whose spider-fauna has as yet been separately published. O. PICKARD-CAMBRIDGE.

BIRDS'-NESTING WITH THE CAMERA.

BRITISH BIRDS' NESTS; How, Where, and When to Find Them. By R. Kearton. Illustrated from photographs by C. Kearton. Crown 8vo. Pp. i.-xx., 1-368. London: Cassell & Co. Price 21s.

THE romance of birds'-nesting exercises a magnetic attraction over many minds. We have clambered along the crags of beetling precipices, creeping on hands and feet where it was impossible to walk; have waded breast-high through swollen torrents; and have spent our nights under the open sky, in order to gratify our passion for gazing on nests that were new to us. Our small share of success affords the happiest reminiscences. So it is with hundreds of enthusiastic ornithologists. Most of them have since regretted that they had no opportunity of photographing the eggs and nests which rewarded their rambles over lonely islets or mist-wrapped hills. It was therefore an excellent idea of Messrs. Kearton to present the public with a series of about one hundred full-page plates from nature. The book is professedly intended as a guide to finding the nests of British birds. We should feel sorry if, by any chance, it gave a new impetus to the destructive custom of forming private collections of the eggs of our native birds. But though we look in vain for any protest against egg-grabbing from our author, Mr. R. Kearton, he has given us a pleasant running commentary on the plumage and habits of many familiar species. His text has the merit of being arranged with lucidity. At the same time, we feel a little regret that the proof-sheets were not corrected by some well known and accurate ornithologist. We should then have been spared the ungrateful task of pointing out that the letterpress requires further revision. For example, if Mr. Kearton was not aware that the female of the dotterel is the brighter of the two sexes, he should have consulted some recent authority instead of stating the exact contrary. Similarly, if he did not know much about the plumage of the partridge, he might have consulted Mr. Ogilvie-Grant's papers on the subject, instead of volunteering the mistaken dictum that the female of this common bird does not assume the dark horseshoe until

MERLIN.

From a photograph by C. Kearton, in "British Birds' Nests," by R. Kearton. Kindly lent by Messrs. Cassell & Co.

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the second year. If a writer chooses to adopt the *rôle* of a popular teacher, it is not too much to expect that he will first consult the literature of his subject; but we would prefer to leave the task of fault-finding to somebody else. The real, transcendent merit of the work lies in the splendid plates with which it is adorned, two of which we are enabled to reproduce through the kindness of the publishers (*see* Pls. III. and IV.). It is easy to take photographs of birds' nests. It is most difficult to secure satisfactory results. Only those of us who have plodded up and down the mountain sides, with camera and tripod on the shoulder, can comprehend the vast amount of trouble needed to obtain such a noble series of negatives as those which Mr. C. Kearton has actually developed. Not only so, but our authors have set to work with a fine, artistic perception of the beautiful. No one can turn over the pages of this volume without feeling grateful to these spirited nest-hunters for the delicious insight into the habits of wild birds which their pictures afford.

H. A. Macpherson.

Dubois on Past Climates.

The Climates of the Geological Past, and their Relation to the Evolution of the Sun. By Eug. Dubois. 8vo. Pp. viii., 167. London: Swan Sonnenschein & Co., 1895. Price 3s. 6d.

The author tells us in the preface that "The present essay is an attempt to explain by changes of the solar heat the great climatic changes of the Geological Past." It is a translation, with some small alterations, of a treatise in German, "Die Klimate der geologischen Vergangenheit, und ihre Beziehung zur Entwickelungsgeschichte der Sonne," which appeared in the beginning of 1893 (Nijmegen: H. C. A. Thieme). In the first part of the book we have a general account of the climates of the past; in the second, a discussion of the "changes in the solar heat as the agency by which the geological climatic changes were brought about." At the outset the author discusses the difficult question of fossil plants and animals as indices of climatic changes in past time; the subject-matter of this part of the essay is practically the same as that treated of in the Sedgwick Essay of 1892.[1] Dubois wisely points out the danger of relying too closely on organisms as safe guides in questions of climate. He writes: "A fixed relation between the climate and the character of the organic world does not exist." After speaking of the spreading of northern forms towards the south, and their gradual acclimatisation to new conditions, he adds: "In this way southern types will always display a certain relationship to the older forms of the northern regions, the cause of which is thus not to be found in a change of temperature in their respective stations." The arctic floras are briefly dealt with, and the familiar conclusions drawn as to Tertiary climates. In speaking of the Carboniferous vegetation, the author refers to the preponderance of marattiaceous ferns among the Coal period Filicinæ; it should be borne in mind, however, that the Marattiaceæ at the present day are very few in number, and confined to tropical regions. It is hardly safe, therefore, to compare the living remnants of this once vigorous and widespread family with the Marattiaceæ of the Carboniferous forests as regards climatic conditions of growth.

Other authors have suggested that the *Glossopteris* flora of the Southern Hemisphere, which appears to have replaced the characteristic Coal flora towards the close of the Palæozoic period, owes its

[1] "Fossil Plants as Tests of Climate." A. C. Seward. London: 1892.

origin to the fall of temperature, of which we have evidence in the widespread glacial deposits of India, Australia, and Africa. Dubois accepts this view, but does not attempt to discuss the probability of this southern flora being the product of a cold climate. It is difficult to recognise in the plants of this period any characters suggestive of arctic or arctic-alpine conditions. A displacement of the poles is given up as untenable. "Should astronomers and physicists raise no objections to a considerable displacement of the poles, the facts observed with regard to arctic vegetation of the Tertiary period still offer sufficient arguments against such a supposition."

To discuss the numerous debatable points touched on in this interesting essay would take us far beyond the limits of the space at our disposal. It is satisfactory to find that no very considerable reduction of temperature is demanded for the Glacial period. The author quotes, and apparently accepts, the opinion of Penck: "A general reduction of temperature of 4°–5° C.—and for a climate damper than the present, of even less—suffices to account for the whole Glacial phenomenon." Assuming suitable geographical conditions, we need not have recourse to such a serious lowering of temperature as is frequently deemed necessary. In the second part, the causes of climatal changes are dealt with at some length, and it is urged that we must look to changes of the solar heat for the cause of the geological changes of climate. An attempt is made to examine the sun's history in detail. "The sun is a star like the thousands which appear at night as luminous specks in the celestial vault, even of relatively small dimensions, and differing from the others merely by its closer proximity to the earth. The substances of which it consists are entirely the same as those of which many other stars are composed." The past history of the sun, its gradual change from a star of the first class to a star of the second class, is compared with the history of geological climates. With the sun in the condition of a white star, says Dubois, organic evolution may have progressed faster than in the Tertiary period, when the light and heat of the sun were already diminishing. This part of the essay contains much that is highly suggestive, and is well worthy of attention by those qualified to estimate the value of the evidence and the author's deductions.

On page 11 we find the not unusual mistake of applying the common name of mares' tails to the Equisetums, and on page 16 *Salisburia* is written *Salisburea*. The statement that Dicotyledons make their first appearance in Jurassic times is not supported by trustworthy evidence; we have no traces of these plants prior to Lower Cretaceous times. The essay may be recommended both as a careful compilation of previously-recorded facts, and as an original contribution towards the causes of climatic changes during the past history of the earth.

A Physical Mantell.

Open-Air Studies: An Introduction to Geology Out-of-doors. By Professor Grenville A. J. Cole, M.R.I.A., F.G.S. 8vo. Pp. xii., 322, with 12 plates and 33 other illustrations. London: Charles Griffin & Co., 1895. Price 8s. 6d.

In spite of the multiplicity of text-books of geology, there has always been room for one which would do for the physical branch of the science what Mantell did for palæontology. A book was wanted that would take the student out-of-doors, and there teach him to observe and reason on the problems of physical geology, just as

Mantell taught the learners of his day where and how to collect fossils. The existing manuals on field-geology are addressed rather to those who know a good deal already, and wish to apply their knowledge in mapping, or else who have to attempt the unravelling of the structure of unexplored regions. These are therefore too dull and concise for most beginners. The elementary text-books are written too much in the form of digests of known facts and current theories to be sufficiently suggestive of observation and thought to the class of students that we have in mind. The present treatises on palæontology are admirable summaries of existing knowledge or popular sketches of life groups; but they have not Mantell's stimulating enthusiasm, and do not send men forth to collect with the same energy as did that author's "Medals of Creation."

Professor Cole's new work admirably supplies this want. It is not a formal systematic text-book, but the student who has travelled across England with it will be more likely to become a useful geologist than one who has read a volume of three times the size. It begins with a chapter on the "Materials of the Earth," which is necessary, and is necessarily dull; but it is the only dry chapter in the book. After having learnt from this the simple properties of rocks and minerals, and how to identify them, the student is recommended to go into some hill country and study "A Mountain Hollow," which is the subject of the second chapter. Here he learns how to examine a section, to study erosion and wind and water, to distinguish between talus, drift, and bed rocks, to watch the formation of clouds, their descent as rain and snow, and to see glaciers at work, or to trace the signs of their former existence. Thus a mountain hollow is made to serve as an object lesson to illustrate many important geological phenomena. In the same way the student is directed to the valley, along the sea-shore, across the plains, to the craters of dead volcanoes, and over granitic highlands. The three final chapters tell him how to read the annals of the earth, narrate how the stratigraphical succession was first determined, and guide him across the stratified "Surrey Hills" and over the contorted Alpine Mountains.

The book is elementary, but contains much which will repay perusal by advanced workers, for it has a charming combination of antiquarian references with the latest results of modern work. Papers are abstracted of which the ink could hardly have been dry when the proofs of this work passed through Professor Cole's hands. And side by side with these are stories of last century speculations and scientific hoaxes. Occasionally, the author has accepted results which are of startling significance, but which are nevertheless hardly likely to be verified. Thus he retains life in the Archæan series, replacing Dawson's *Eozoon* by Cayeux's Radiolaria, though the latter are almost certainly as inorganic as the former. He accepts man as Lower Pliocene in age (p. 241) on the strength of Noetling's testimony. Occasionally, the conclusions of physical evidence have been adopted without reference to the teaching of palæontology or neontology as, *e.g.*, in reference to the formation of the Caspian Sea (p. 145). But these are details; in his use of recent references the author usually shows great discrimination and caution. The illustrations of geological phenomena quoted are always apt and refreshingly original; for the book is the work of a man who loves the open air and has travelled far and wide with his eyes open, and he has thus been able to draw his examples from all sorts of odd corners scattered over the north-western quarter of Europe.

There is one further feature in the book which would alone make

its perusal pleasurable and profitable, and that is the elegance of its English. Professor Cole's papers have always been characterised by their accuracy and ease of expression. Throughout the present volume we come on picturesque touches which are most welcome to the student in a town. They bring back reminiscences of countries where "the granite hills flush rose-red in the glow of evening," of passes "where there is still a chance of startling an eagle in some hollow filled with the sea-mist," of "clouds creeping across the wind-swept *cols*," and of "the soft sheets of mist stealing across the hollows of the fen."

The book is worthy of its title; from cover to cover it is strong with bracing freshness of the air of the mountain and the field, while its accuracy and thoroughness show that it is the work of an earnest and conscientious student.

Fungi in General.

Introduction to the Study of Fungi. By M. C. Cooke. Pp., x., 360; 148 figs. London: A. & C. Black, 1895. Price 14s.

This veteran writer on Fungi has given to the world such a number of books on this subject, and on so many others, that he probably does not claim at this stage of affairs any extraordinary degree of accuracy or enlightenment. If the present volume be judged by the standard of an ideal book on Fungi—the nearest approach to which for its time is De Bary's famous "Comparative Morphology," now getting out of date; if it be judged by what a great writer might have done, or an ordinary well-informed mycologist might have written, one would say it is not a particularly good book. If it be judged by its author's previous performances, such as "Fungi: their Nature, Influence, and Uses," in the International Scientific Series, one would say it is comparatively a good book. It marks an advance in the author's views. Time has mellowed his violent feelings towards some burning questions of the last twenty years, and he no longer employs his fine talent in abuse of most things new. Far be it from us to do other than praise him for his repentance in some matters, late though it be, and carefully modified and guarded as well. He has always fought a good fight, observed the rules of the game in all his contests, and the present writer, who is also growing older, is glad to welcome this approach of an old friend and antagonist.

The great merit of this book is not a new one. Dr. M. C. Cooke has always written well, and he writes this book well. It is not dry. It is well printed and indifferently well illustrated—there is considerable inequality in the style of the cuts. The author has read a good deal in the way of recent morphological literature—though manifestly not enough, nor carefully enough. Still, it is a great advance on his previous books, and may be recommended to the large class of readers who shrink from harder reading in English or German. G. M.

Fungi in Brazil.

Protobasidiomyceten. Untersuchungen aus Brasilien. By Alfred Möller. 8vo. Pp. xiv., 179, 6 plates. Jena: Gustav Fischer, 1895. Price 12s.

Dr. Möller's account of his researches on fungi in the Tropics has now reached the third volume, and, if possible, the interest of the record increases. During the three years of his residence in Brazil he made 9,000 microscopic cultures in his laboratory, and such indefatigable work has been amply rewarded. His attention all

along was specially directed to the Protobasidiomycetes, under which Brefeld has classified all those lower forms of the Basidiomycetes that have divided basidia. The Autobasidiomycetes with undivided basidia include nearly all the larger fungi, the mushrooms, toadstools, etc., and are to form the subject of another paper.

The Protobasidiomycetes have been further subdivided by Brefeld into two groups, distinguished by the form of the basidium. The Auriculariaceæ, brown, leathery, wrinkled fungi, popularly known as "Jew's ears," represent one type: they have long upright basidia divided by transverse walls into four cells, and each cell gives rise to one spore. In the other group, the basidium is more round or egg-shaped, and it is divided lengthwise, like the quarters of an orange. This type prevails in the Tremellineæ, very slimy, gelatinous fungi that grow mostly on decaying wood; there is a long outgrowth or sterigma from the apex of each cell, on the top of which the spore is borne. The reason for this form of fruiting is very apparent, as the spores are thus lifted above the gelatinous covering and their dissemination is secured.

One of Möller's most interesting discoveries was that of *Sirobasidium brefeldianum*, which forms a transition from one to the other of these types. The fungus occurs on dead branches, and looks like little white pearls or drops of water. The basidium is an oval-shaped cell, which has only one somewhat slanting division and two spores without sterigmata, one borne near the apex of the basidium, the other lower down. After the spores are ripe, the basidium shrivels up and the cell immediately beneath develops in turn as a basidium, and thus a row of such shrivelled, empty cells could often be seen surmounting a spore-bearing basidium. A very similar fungus had been found in Ecuador by Lagerheim, and was described by Patouillard in the *Journal de Botanique* for 1892, under the generic name *Sirobasidium*. In the Ecuador plant, the basidium was divided into four cells by longitudinal walls, and the four spores were borne at the apex, also without sterigmata. Here, too, the succession of basidia in a long row was similar to that of *Sirobasidium brefeldianum*, the Brazilian species.

In the preface to this volume, Dr. Möller quotes, with entire approval, Brefeld's statement that to classify and name fungi belonging to the Protobasidiomycetes, it is necessary to rely chiefly, if not alone, on spore culture and development. It was surely rash, then, on Möller's part, to accept the work of Patouillard, who merely described the fungus as he found it. Life is scarcely long enough to watch how the spores grow in every fungus that we may desire to classify. Möller himself has not found such knowledge essential, or he would have laid aside Patouillard's *Sirobasidium* with a query.

The researches of Van Tieghem, and subsequently of Brefeld, have resulted in placing the Uredineæ among the Basidiomycetes near to the Auriculariaceæ, and therefore among the Protobasidiomycetes. The Uredineæ are all parasites, and form the "rusts" that work such havoc among cereal crops. The brown and black streaks and patches that cover the grasses in autumn are formed of innumerable little two-celled brown spores, which are scattered by the wind, and are capable of resisting extreme cold. In spring, they germinate and reproduce the fungus. It is at the stage of germination that the fungus harks back to its kindred, and the germinating filament is a true basidium exactly like that of *Auricularia*. It is divided into four cells which produce the spores, and these in turn grow out into the filaments of the new plant. Dr. Möller does not delay long over this group, but

he describes a new genus, *Saccoblastion*, which he considers a closely allied form. It was found, in moist weather, a thin tangle of hyphæ, on decaying wood and bark. The parent cell of the basidium in this fungus forms a large, drooping, pear-like outgrowth, whence the name *Saccoblastion*. Möller considers this outgrowth to be the equivalent of the two-celled brown spores in the Uredineæ. In the one case there is a mere thin-walled protrusion from the vegetative cell, in the other there is a spore which separates from the parent plant and reproduces it. The homology is, to say the least, very startling and far-fetched.

Two curious genera have been added to the Tremellineæ, viz., *Protomerulius* and *Protohydnum*. The first-named bears a striking resemblance to *Merulius*, the well-known "dry-rot"; *Protohydnum* grows exactly like a *Hydnum*, the under-side being studded with tooth-like projections nearly a quarter of an inch in length. Both these fungi have, however, the characteristic, longitudinally-divided basidium of the Tremellineæ, and only in outward form simulate plants of the more advanced type of the Autobasidiomycetes.

Dr. Möller has added six new genera and twenty-eight new species to the Protobasidiomycetes; and in the forms which he has found he has happily been able to trace a gradual development in the formation of the hymenium, from plants with scattered isolated basidia to those in which the basidia grow in a definite fruiting layer. The thanks of all students of fungi are due to him for this most interesting account of his labours. A. L. S.

MOSSES AND FERNS.

THE STRUCTURE AND DEVELOPMENT OF THE MOSSES AND FERNS. By Prof. Douglas H. Campbell. Pp. viii., 544. London: Macmillan & Co., 1895. Price 14s.

PROFESSOR CAMPBELL is well-known as the author of a number of important papers which are concerned, for the most part, with plants and problems connected with the Archegoniatæ; thus a work from his pen dealing with the cryptogamic department of this great series of plants at once arrests attention and arouses interest. And it is not too much to say, at the outset, that it forms probably the most important treatise of its kind since the publication of the magnificent and fundamental researches of Hofmeister some forty years ago.

The too often neglected group of Muscineæ, and especially the section of the Hepaticæ, meet here with far more adequate recognition than has, unfortunately, become customary. Representatives of the various orders of Liverworts have been investigated by the author; but it must not be imagined that a general and comparative treatment has been lost sight of because some forms are selected as special types. We notice with satisfaction that to *Targionia* is given a due prominence among the Marchantiaceæ. Its comparative simplicity well fits it to serve as a key to the extremely complex structure exhibited by the more familiar genera.

The somewhat detailed treatment of the Anthoceroteæ is a good feature of this part of the book. The group has acquired a position of considerable importance, inasmuch as it not improbably indicates the sort of phylogenetic path along which the ancestors of the vascular cryptogams have travelled.

With the chapter on the mosses proper we confess we are not quite so well pleased. Our old friend *Funaria* is exhaustively studied, but we should have liked to see interesting forms like *Buxbaumia*, *Diphyscium*, *Splachnum*, or *Fissidens* receive far more attention than they have actually met with. In this, as in other parts of the book,

we think the following-up of cell lineage has been too exclusively pursued, at the expense of a wider morphological survey of the mature forms and of the numerous adaptations to circumstances of the environment so fully illustrated by these plants.

Naturally, the vascular cryptogams occupy the largest share of the book. Here Professor Campbell works out the views, which he was the first to propound, and which have been largely accepted by many other botanists, especially in this country, viz., that the eusporangiate ferns are older, phylogenetically speaking, than the leptosporangiate forms. The author also insists that it is among the Hepaticæ, rather than the Musci, that their lower affinities are most naturally to be discerned.

The Ophioglossaceæ are regarded as the most primitive type, and as being most nearly related to such a form as indicated by *Anthoceros*. The author works out this point, so far as the evidence goes, both in the sporophyte and the gametophyte, and he lays considerable weight on the results of comparisons between the antheridia and archegonia of the two groups respectively. We confess that, as regards the antheridia, we do not think that a very good case has been made out, nor indeed does Professor Campbell express himself on this point without reserve. The antheridia of the Anthoceroteæ are very peculiar, and need some manipulation in order to be forced into line with those of the ferns. However, it must, of course, be remembered that, in any case, it is not *Anthoceros* itself which is contemplated as lying in the line of direct descent; it only serves to give the idea of a generalised type to which the ancestors of the present higher plants may have roughly conformed. The argument drawn from the comparative study of the archegonium is much more convincing, and the author's suggestion that the whole archegonium of a vascular cryptogam corresponds only to the axile series of the analogous body in liverworts we regard as a peculiarly happy one.

The leptosporangiate ferns, so far, at least, as concerns the homosporous members, form a well-defined group, their relationship with the eusporangiate series being indicated through the Osmundaceæ. As regards the heterosporous families, Professor Campbell considers the Salviniaceæ as a possible offshoot from the already divergent Hymenophyllaceæ, while the Marsiliaceæ are assumed to be derived from the Polypodiaceæ. We should ourselves, however, be disposed to remove the Marsiliaceæ much further from any existing homosporous type than Campbell has done; the very strongly-marked degree of reduction in their gametophyte and the great peculiarity of the sporophyte, both in structure and in position, seem to us to be facts which indicate that, if they are really related to the Polypodiaceæ at all, their affinity is at best but an extremely remote one. It is, we venture to think, not impossible that they may turn out, after all, to be more nearly allied with some eusporangiate prototype resembling the Ophioglossaceæ. It is not *à priori* impossible that the present character of their sporangia may well have been developed quite independently of any real descent from primitive leptosporangiate ancestors. Heterospory has appeared more than once in the alliance of vascular cryptogams, why not the leptosporangiate character also?

The general summary at the end of the book is a fine piece of work. The full discussion of the mutual relationships existing between the various subdivisions of the cryptogamic archegoniatæ, and between these and the higher spermophytes, is full of interest and suggestive thought from first to last. Professor Campbell regards the phanero-

gams as representing the outcome of at least two separate lines of descent, the gymnosperms, or at any rate the Coniferæ, being referable to a lycopodiaceous stock, while the source of the angiosperms is to be sought for among the eusporangiate ferns.

It is impossible, while respecting ordinary limitations of space, to do complete justice to an important work like that now under review; but enough has been said to show that no one who wishes to realise the present position of plant morphology can afford to neglect Campbell's book. On the other hand, we cannot blind ourselves to some of its more obvious defects. We have already referred to the almost entire lack of "bionomic" element, and we also think that a more extensive treatment of the variation that occurs within small circles of affinity would have been appreciated. But it is against the almost complete neglect of palæontology that our most serious complaint is directed. Quite the best and most important of the palæophytological work has been done in the department of the archegoniate plants, and we imagine there will be but few readers of Dr. Campbell's book who will not feel disappointed at the perfunctory manner in which this branch of the subject has been dismissed. This omission, we may hope, will be remedied in future editions; it constitutes at present a defect in what is otherwise a fine book. J. B. F.

New Serials.

The U.S. Weather Bureau has started a monthly entitled *Climate and Health*, edited by Dr. W. F. R. Phillips. By means of tables and charts it discusses the relations of disease and climatic conditions.

A quarterly, entitled the *Journal of Experimental Medicine*, is announced by Appleton & Co., New York. It is devoted to original investigations in physiology, pathology, bacteriology, pharmacology, physiological chemistry, hygiene, and medicine. The editor-in-chief, Professor H. Welch, of Johns Hopkins University, is assisted by twelve other well-known Americans. A similar journal has been begun in Russia, under the editorship of Professor W. W. Podevyssotzky, of Kieff. It is entitled *Russisches Archiv für Pathologie, Clinische Medezin und Bacteriologie.*

We note a monthly, entitled *Devonia*, edited by E. W. W. Bowell and E. H. Bazeley, price 1s. 6d. per quarter, post free. Communications may be addressed to the editors at Huntsham, Bampton, North Devon. The paper is at present reproduced from manuscript, apparently by the autocopyist. It contains natural history notes of local and general interest. The energy of the conductors is praiseworthy, though it might be directed with more utility towards the improvement and aid of existing journals such as *Science Gossip*, which, if not dead, is hibernating.

Literature Received.

Origin of Plant Structures, G. Henslow: Kegan Paul. Telescopic Astronomy, A. Fowler: Philip. De Gewervelde Dieren, T. C. Winkler: Haarlem. Faune de France; Coléopteres, A. Acloque: Baillière. N. American Fauna, no. 10: U.S. Dept. Agriculture.

Igneous Rocks of the Giridil Coal-field, Holland and Saise: *Rec. Geol. Survey India.* The Botanical Outlook, J. M. Coulter. Botanical Studies, xxiv.–xxviii.: Minnesota State Botanist. Surviving Refugees of Ancient Antarctic Life, C. Hedley: *Trans. Roy. Soc. N.S.W.* Perissodactyls of the Lower Miocene, Osborn and Wortman: *Bull. Am. Mus.* Some Factors in Evolution of Adaptations, G. Haviland. Natural History and Scientific Book Circular, including works from the Libraries of Sir G. B. Airy and A. C. Ranyard: Wesley & Son.

Knowledge, Dec. and Jan. Review of Reviews, Jan. Nature, Dec. 19, 26, Jan. 2, 9. Literary Digest, Dec. 14, 21, 28, Jan 4. Revue Scientifique, Dec. 21, 28, Jan. 4, 11. Irish Naturalist, Jan. Revue générale Sciences, Aug. 15, 30, Sept. 15. Feuille jeunes Naturalistes, Jan. American Journ. Science, Jan. Victorian Naturalist, Oct. Science, Dec. 13, 20. Scottish Geographical Mag., Jan. Westminster Review, Jan. Naturen, Nov. and Dec. Nature Notes, Jan. American Naturalist, Dec. and Jan. The Naturalist, Sept., Nov., and Jan. American Geologist, Nov., Dec., and Jan. Botanical Gazette, Dec. Biology Notes, Nov. and Dec. The Photogram, Jan. L'Anthropologie, Nov. and Dec. Naturæ Novitates, Dec.

OBITUARY.

WE regret to announce the death of Mr. HUGH MILLER, F.R.S.E., F.G.S., son of Hugh Miller (author of "The Old Red Sandstone," "The Testimony of the Rocks," etc.). Mr. Hugh Miller, who was born July 15, 1850, joined the Geological Survey in 1874, and was engaged at first in surveying portions of Northumberland. Later on he was transferred to the Survey in Scotland, and mapped some of the regions around Cromarty, rendered classic by the observations of his father. He then proceeded to Sutherlandshire, and was engaged in surveying the Eastern Schists, the Old Red Sandstone, and the Glacial Drifts. He was the author of a little work on "Landscape Geology," and of papers on the Glacial Phenomena of Northumberland. He died January 8, 1896.

IN our article on the Perthshire Museum of Natural History considerable mention was made of Colonel HENRY MAURICE DRUMMOND HAY, of Seggieden. Our readers will regret to hear that he died at his family residence on January 3, at the age of 82. He had one of the best private natural history collections in the country, it being especially rich in birds and their nests. While in Bermuda with his regiment, the 42nd Royal Highlanders, he made an exhaustive collection of fishes, and his reports and drawings were sent to the American Fishery Commission in 1860, and were highly commended. His loss will be greatly felt by the Perthshire Society of Natural Science.

THE *Victorian Naturalist* gives the following account of Mr. J. BRACEBRIDGE WILSON, M.A., F.L.S., who passed away at the age of 67 on October 22 last. "In his position as head master of the Church of England Grammar School, Geelong, for the past twenty-two years, he had made hosts of friends all over Australia, who will deeply feel his loss. Among biologists his name will long be remembered as an enthusiastic collector of the sponges and algæ of the vicinity of Port Phillip Heads, where he was accustomed to spend all his holidays dredging for fresh material to be forwarded for working out to such friends as the late Dr. Macgillivray, of Bendigo, Baron von Müller, Professor M'Coy, F.R.S., Melbourne University, or Dr. Dendy, now of Christchurch, New Zealand; or through them to the leading specialists of England or the Continent. He was for a

number of years a member of the Field Naturalists' Club of Victoria, and took considerable interest in the work of the Geelong Field Naturalists' Club."

The Rev. J. G. Morris, whom the *American Naturalist* describes as one of the fathers of American entomology, died in Lutherville, near Baltimore, on October 10, aged 92. His catalogue of the lepidoptera, published in 1860 by the Smithsonian Institution, and his Synopsis of Diurnal and Crepuscular lepidoptera are the works by which he was best known to entomologists.

A full account and a bibliography of Johannes Frederik Johnstrup, of Copenhagen, whose death we announced in our last February number, p. 138, is contributed by Victor Madsen to vol. xvii. of the *Förhandlingar* of the Geological Society of Stockholm, pp. 85–96.

Other deaths recorded are those of: E. A Wünsch, a local geologist of note, who spent his earlier years in the neighbourhood of Glasgow, and his later ones in Cornwall, in December, 1895; Rev. W. Thompson, the author of "Florula Sedbergensis," who was born at Mallerstang in 1843, and died at Sedbergh on June 6, 1895; M. S. Bebb, of Rockford, Illinois, well-known for his work on the willows, in California, on December 5 last; Dr. F. P. Porcher, Professor of Materia Medica and Therapeutics in the Medical College of the State of South Carolina, and author of numerous works on pharmaceutical botany, at Charleston, S. C., on November 19; General O. v. Radoszkowski, the hymenopterologist, in Warsaw, last May; A. P. Kosticher, Director of Agriculture in Russia, who created the first laboratory in Russia for the study of soils and agricultural products, and wrote a large work on the black earth of Southern Russia; Dr. Peter S. Popow, extraordinary Professor of Physiology at Dorpat University; Dr. Teichmann, formerly Professor of Anatomy at Cracow; Dr. A. von Brunn, Professor of Anatomy at Rostock University, on December 10, aged 46; Dr. K. B. Schiedermayr, the botanist, in Kirchdorf, Austria, on October 29; Professor G. Krabbe, plant-physiologist, formerly of Berlin, at Wonsahl, near Ibbenbüren, in Westphalia, on November 3, aged 40; O. Borchert, African explorer, on November 13, in Ludwigslust; Dr. Paolo Galardi, Professor of Natural History in the Lyceum of Siena, on December 8, 1895; Dr. Achille Quadri, Professor of Zoology at Siena University, on December 17, 1895; the Polish botanist, Dr. F. Berdau, on November 27; and Antonio de Castilio, founder and director of the Geological Survey of Mexico, who died in the City of Mexico on October 27.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

The following appointments have been announced:—Colonel G. T. Plunkett, R.E., Secretary of the Royal College of Science for Ireland, to be Director of the Science and Art Institutions in Ireland; A. W. Rogers, of Christ's College, Cambridge, to be Assistant Geologist on the Geological Survey of Cape Colony; W. L. Sclater, to be Curator of the South African Museum, Cape Town; Dr. Hürthle, to be Extraordinary Professor of Physiology at Breslau; Dr. K. G. Hüfner, Professor of Organic and Physiological Chemistry at Tübingen, to succeed the late Professor Hoppe-Seyler at Strassburg; Dr. A. Froriep, to be Ordinary Professor of Anatomy at Tübingen; Dr. W. Roux, of Innsbruck, as Professor of Anatomy in Halle University; Dr. Karl Müller, of Berlin, as Professor of Technical Botany to the Technical School of Charlottenburg; Professor N. Kudnetzoff, as Director of the Botanic Garden of Dorpat University, with N. Busch as Assistant-Director; Dr. J. Ritzema Bos, as Professor of Plant-pathology at Amsterdam University; Dr. N. V. Ussing, as Professor of Mineralogy at Copenhagen University; Dr. Carlo Fornasini, to be Assistant in the Geological Museum of Bologna University; Professor Achille Russo, to be Lecturer in Natural History at the Technical Institute of Melfi; G. Nappi, to be Professor of Natural History at the Lyceum of Caltanisetta; F. Pierucci, to be Professor of Natural History at the Lyceum of Rieti; Dr. A. Bertino, to be Assistant in Zoology, Physiology, and Comparative Anatomy at Sassari University; Giuseppe Boccino, to be Assistant in Natural History and Agriculture at the Technical Institute of Udine; E. Ficalbi, of Cagliari, to be Professor of Zoology in Messina; Professor Gaetano Pittalunga, to be Instructor in Natural History at the Lyceum of Savona; Dr. Theobald Smith, late Chief of the Division of Animal Pathology in the U.S. Department of Agriculture, to be Lecturer at Harvard, and Bacteriologist to the Massachusetts State Board of Health; he is succeeded by Dr. V. A. Moore, who has for assistant Dr. P. A. Fish; Dr. Lucien M. Underwood, to be Professor of Botany in the Agricultural and Mechanical College at Auburn, Alabama; Mr. F. S. Earle, to be Assistant in the Division of Vegetable Physiology and Pathology in the U.S. Department of Agriculture, in place of J. F. James; F. L. Washburn, to be Professor of Zoology at Oregon State University; Dr. H. Nichols, to be Lecturer in Psychology, and Dr. James Ellis Humphrey, to be Instructor in Botany, at Johns Hopkins University; Dr. J. P. Lotsy, formerly of Johns Hopkins, to assist Dr. Treub at the Buitenzorg Gardens, Java.

Mr. C. A. Barber has been appointed temporary lecturer in botany at Cooper's Hill, in order to fill the vacancy caused by Professor Marshall Ward's transference to Cambridge. We understand that changes in the constitution of the college are contemplated, and, until the new scheme is settled, no permanent appointment will be made. We trust, however, that the position of botany in the curriculum will not be lowered in any way.

Mr. Harry Page Woodward has resigned the post of Government Geologist to West Australia, and has taken service with the well-known firm of Bewick, Moreing & Co., of Coolgardie and London.

On January 11, the Academy of Sciences at St. Petersburg elected Professor James Hall, of Albany, an Honorary Foreign Member, and Professor Ray Lankester and Mr. C. D. Walcott among its corresponding members.

On the occasion of Sir Henry Acland's retirement from the Regius Professorship of Medicine in the University of Oxford, a meeting was held in the hall of All Souls' College, when the Vice-Chancellor presented Sir Henry with an illuminated address, and with a list of 400 subscribers to a sum of some £3,000, which will be devoted to the enlargement of the Sarah Acland Home for Nurses. A bust

of the professor is to be placed in the University museum, and "will remind present and future students that he was the chief mover in the creation of the museum and the developmeut of the Radcliffe Library as a centre of physical science in Oxford."

THE Fishmongers' Company has given to the City and Guilds Technical College, Finsbury, a scholarship of £60 per annum in memory of Professor Huxley. This is to be held three years by any scholar of the Finsbury College who has given evidence of high scientific attainments, to enable him to proceed to the Central College at Kensington

THE million dollars demanded by Mr. J. D. Rockefeller, as a condition of his own gift of the same amount to Chicago University, has been produced by Miss Helen Culver. The sum is assigned to the biological department. It is probable that a school of medicine will be established.

WE learn from the *Manchester Guardian* that considerable improvements have recently been made in the museum and art galleries at Peel Park, Salford. We are not so much concerned with the additions to the picture galleries and those illustrating manufactures, as with gallery C, devoted to the ethnographical section, and rooms F and G, which have recently been handed over to the geological section. The ethnographical specimens have lately been arranged on a system such as will enable them to appeal more effectively to the public intelligence. Beginning with prehistoric man, and working up through palæanthropic and neanthropic ages into the early historic, mediæval, and recent eras, the specimens terminate at one place with flint-and-steel implements. The specimens following are grouped geographically, and illustrate the weapons and utensils, clothing, etc., used by different races all over the world. The geological rooms have been put in order by Herbert Bolton, of the Manchester Museum, Owens College. The classification of the specimens has brought out a large amount of material formerly stowed away and hardly recognised as existing. Consequently, two series of wall-cases, each about fifty feet in length by six feet high, have been added. These contain larger specimens which are not well adapted, on account of their size, for exhibition in the table cases. The Coal-measures, on account of their importance to the district, take a large amount of space for their illustration, but choice specimens are much needed of coal-plants, most of those formerly possessed by the museum having fallen to pieces. The general arrangement of the specimens is stratigraphical, and Sir Henry Howorth, whose influence, perhaps, is traceable here, will be glad to see that the last table-case, which contains specimens illustrative of the most recent phase of geological history, has been arranged in agreement with the contents of a special case in the anthropological gallery, the latter arranged by the curator as an introduction to the general anthropological collections. By this means, it is hoped the visitor will carry his studies from either of the sections to the other, while the arrangement emphasises the general continuity of prehistoric and historic time.

A special department of practical geology illustrates the chief coals of the Lancashire coalfield, and a small selection of other coals, together with a series showing the chief stages in the process of change from peat to coal. There is also a section of building-stones, which is intended to form a group more especially illustrative of those used in Salford and Manchester, and, whenever possible, two specimens of each will be shown, one fresh from the quarry, the other from some building where it has been exposed to the action of the weather for a number of years. In the latter case, the aspect of the building-stone will be indicated, the number of years it has been weathered, and the deterioration which has resulted. One feature of interest to the geologist is the extensive use of photographs. Most of these show the action of geological processes over large areas, as, for example, the destruction of cliffs, the formation of river valleys, and the appearance presented by eruptive rocks. The photographs, like the specimens, are mainly illustrative of British geology, although, for the sake of completeness, foreign specimens are being introduced to represent those periods which have left no traces in the British Isles. The work is being rapidly pushed forward, and it is hoped that the rooms may be thrown open again to the public in March.

A Museum of Anatomy and Surgery has been founded in St. Petersburg. For its use, a building on the banks of the Neva has been purchased by the Pirogoff Chirurgical Society, and this will be reconstructed with the aid of 60,000 roubles bequeathed to the Society by the late Countess Musin-Pushkin, and 30,000 provided by the Government. The museum is to be arranged on the lines of that of the Royal College of Surgeons.

The following is the list of awards of the medals and funds made by the Geological Society, London, for the present year:—Wollaston Medal, Professor E. Suess; Murchison Medal, Mr. T. Mellard Reade; Lyell Medal, Mr. A. Smith Woodward; Wollaston Fund, Mr. A. Harker; Murchison Fund, Mr. P. Lake; balance of the Lyell Fund, Dr. W. F. Hume and Mr. C. W. Andrews; Barlow-Jameson Fund, Mr. Joseph Wright, of Belfast, and Mr. Storrie, of Cardiff.

The Union of Irish Field Clubs is undertaking a Directory of Irish Naturalists. which will doubtless forward the good work being accomplished by the Union, and be useful to their English colleagues.

We learn from *Nature* that it is proposed to form a society to bring together more closely those who have taken up reptiles as their hobby, and it is hoped that by this means interest may be kept up and mutual help secured by all concerned, Dr. Arthur Stradling has consented to become President. In order that a working basis may be secured at once, those who intend to become members should communicate with the Secretary, Rand Rectory, Wragby, Lincolnshire.

The third International Congress of Psychology will be held at Munich from August 4 to August 7 in this year. The President Elect is Professor Stumpf, of Berlin; the Vice-President is Professor Lipps, who is also chairman of the reception-committee. All persons, whether male or female, who desire to further the progress of psychology are invited. The subscription is 15s., which entitles members to the daily journal and to a copy of the report. All who intend to read papers are requested to send their names and subjects to the Secretary, Dr. Freiherr von Schrenk-Notzing, Max Josephstrasse 2, Munich, before May 15.

A meeting of the Farmers Institute was held at the New Mexico College of Agriculture from January 2 to January 4, when various papers dealing with the practical and scientific aspects of agriculture were read. The chair was taken by Professor S. P. McCrea, Director of the New Mexico Agricultural Experiment Station.

A company has been formed in Paris in order to extend the methods of Pasteur in medicine. Messrs. Duclaux, W. Roux, J. B. Pasteur, and Radot form the executive council. A monument is to be erected at Melun, near Fontainebleau, to commemorate Pasteur's experiments in vaccinating sheep suffering from anthrax, which were first made in that district. It is also proposed to erect a statue of the deceased investigator in some public place in Paris.

According to the *Irish Naturalist*, Professor Sollas, of Dublin, will leave in March for Sydney, to take charge of an expedition that is being despatched to make deep borings in a coral atoll. The scheme, which is supported by a strong scientific committee, has been financed by the Royal Society to the extent of £800; and the Government are placing a gunboat at the disposal of the party, to convey them from Sydney to Funifuti, in the Central Pacific, which has been selected as the scene of operations.

By the kindness of Dr. Henry Woodward we are able to announce the safety of Dr. Forsyth Major and his companion Robert. They were at Antananarivo on December 1. Dr. Major states that the country is somewhat unsettled around Vakinanzaratra, but improvement is rapid. Both have enjoyed good health, and worked in the forest right through the rainy season of last year without harm.

A COPENHAGEN correspondent of the *Daily Chronicle* states that a Danish scientific expedition, which has for its object the exploration of the Pamir district and Kafiristan in Central Asia, will probably leave Copenhagen during the month of March. The expedition will have for its leader Lieutenant Olafsen, and its final equipment will be completed at Samarkand.

AT a meeting of the Royal Scottish Geographical Society held in Edinburgh on December 19, Mr. Borchgrevink lectured on his recent voyage to the Antarctic regions. Dr. John Murray, who occupied the chair, spoke of the interest connected with a scientific expedition to the South Polar Seas, and regretted that Mr. Goschen felt unable to recommend his colleagues to spend money or spare a vessel for such an undertaking while the East was in its present disturbed state. The Royal Geographical Society of London were considering the practicability of sending out an expedition, independently of the Government, at an early date. If the money necessary could not be collected, a party might perhaps be sent out with an expedition that was proposed for killing blue whales. With £5,000 the chairman believed that arrangements might be made to send out twelve men with a commercial expedition, who might be landed on the Antarctic continent and taken off in the following year.

MR. FRANK H. CUSHING, of Washington, is in charge of an expedition to St. Augustine, in Florida, for the purpose of exploring the islands of Florida Keys, and of studying the tumuli recently discovered there. These are characterised by numerous ornaments made from sea-shells.

PROFESSOR DYCHE, who was on the Peary Relief Expedition, has received such offer of support as enables him to project another journey along the west coast of Greenland in the direction of the North Pole.

WE are exceedingly glad to note a new departure by H.M. Stationery Office. This is the reproduction of coloured geological maps by printing instead of by hand as has hitherto been the case. We have received a copy of sheet 12 of the so-called Index Map of the Geological Survey of England and Wales, on the scale of four miles to the inch. It is the first colour-printed map ever issued by our Survey, which we should imagine was alone among Surveys in sticking to the old-fashioned process. This sheet, which includes the country round London, from Ipswich and Bedford on the north to Petersfield and Battle on the south, is sold for 2s. 6d., whereas the same coloured by hand costs 10s. 6d. The appearance of the map is far smoother, and the printing more legible, especially where it underlies the darker shades. To this must be added the advantage of greater accuracy, since hand-colourists are not immaculate. Doubtless the sale of this map will encourage the Stationery Office to proceed with the rest of the series, and even to extend the method to the ordinary one-inch scale maps.

PRINCE ALBERT of Monaco has rendered to the Paris Academy of Sciences an account of the last expedition of his yacht "Princesse Alice," which lasted from June 17 to August 12, and extended from Lisbon to the Azores and back again to Havre. Besides taking many deep soundings and dredgings, the yacht captured numerous animals at the surface by various means, the latter operation resulting in the discovery of many new species of animals of all orders. The most interesting part of the voyage was, however, the death of a sperm-whale on July 18. In its last convulsions the whale threw up a large number of cephalopods of great size and possessing many remarkable characters. One of these was particularly curious owing to the polygonal scales which clothed its upper and under surfaces, and were of a character hitherto unknown. The whale also furnished to the zoologists on board a large number of parasites, and served the photographers as a subject for numerous interesting plates.

CORRESPONDENCE.

Reply to Criticism upon "Notes from the Geological Laboratory" of the Johns Hopkins University.

The criticisms contained in your last number (p. 4) upon the methods of specific description employed in the "Notes from the Geological Laboratory," arise from an entirely erroneous conception of the character of the articles presented. These articles were intended simply as reviews of investigations carried on during the past year at the geological laboratory, and were not intended to give more than an outline of results. The full reports, and, to a large extent the plates also, have been already prepared for final publication and will shortly appear from the press. No pretence was made to give full diagnoses of the forms described, while it was distinctly stated that "The species referred to in the accompanying list have been largely figured, and the plates, together with the fuller descriptions of the forms, will be found in the forthcoming Government report."

Your stricture regarding the multiplication of new species, which is applicable to work in areas where a sufficient number of characteristic forms has been already described, does not fit a case like that of the Eocene of the Middle Atlantic slope, where the fauna is practically unknown, and where the amount of information has been wholly insufficient for stratigraphical purposes. The synonymy of the few species described by previous writers from this area has not been ignored, as an examination of the review will show.

The *Circulars* of the Johns Hopkins University have long afforded the scientific departments the means of bringing forward, in the form of preliminary notices and reviews, the investigations which are being conducted in the laboratories and have never been regarded as a permanent medium of publication.

Johns Hopkins University. William B. Clark.

[We welcome Professor Clark's letter, as it affords additional argument in support of our remarks on "The Preliminary Notice" (*see antea*, p. 73).—Editor, Nat. Sci.]

Reducing Divisions in the Formation of the Polar Bodies.—A Correction.

The review of my "Atlas of Fertilisation," published in Natural Science for December, 1895, contains a misinterpretation of my views regarding the phenomena of maturation that I trust you will allow me to correct. The passage reads :—

"It is clear that the distinction between reducing divisions and ordinary divisions suggested by Professor Weismann cannot be applied to the formation of the polar bodies as interpreted by Professor Wilson" (p. 378).

There is nothing, I think, in the brief account of maturation given at pp. 9, 10 of the Atlas, or in the accompanying figures, to bear out this statement. The ultimate problem of reduction, as Boveri long since pointed out, lies in *the mode of origin of the tetrads*, of which nothing is said in the Atlas, since my purpose was only to give an outline of the broader features of maturation. The work of Häcker and Rückert seems to leave no doubt that in the copepods the origin of the tetrads is such that the division of the dyads during the formation of the second polar body must be interpreted as a "reducing division" in accordance with Weismann's earlier views. This result is diametrically opposed to the conclusions of Boveri, Hertwig, and Brauer in the case of *Ascaris*, and cannot at present be reconciled with them. It was on account of this contradiction that the origin of the tetrads was not critically considered in the Atlas. The account there given, which considers

only the later stages of maturation, is consistent with either Brauer's or Rückert's view, and I did not wish to complicate the subject by the introduction of theoretical considerations relating to bivalent chromosomes, reducing divisions, and the like.

Columbia College, EDMUND B. WILSON.
December 9, 1895.

"THE GLACIALISTS' MAGAZINE."

IN the January number of NATURAL SCIENCE references are made to this magazine and myself, upon which I would make the following remarks.

Overwork and ill-health at the close of 1894 and beginning of 1895 brought my work on the magazine into arrear, and when my health broke down completely, last March, a committee was appointed to relieve me of every task that could be deputed. Unfortunately, that committee made a grievous mistake in regard to the distribution of the magazine, and it was not until my return home, after an absence of more than three months, that I learned that, though copies had been sent to the members of the Glacialists' Association in June or early in July, up to and including the first part of vol. iii., none was sent to the publisher. This defect was immediately rectified, and the September part was also sent, which seems to have in some way escaped your notice. [Not at all; see p. 62.—ED., NAT. SCI.]

The *Glacialists' Magazine* is now published quarterly at Lady-day, Midsummer, Michaelmas, and Christmas. The Christmas number was issued on December 31.

Chapel Allerton, PERCY F. KENDALL.
January 13, 1896.

[We have already announced the change in period of publication of this valuable magazine, and we sincerely hope that the easier labours of a quarterly may spare Mr. Kendall's health; but should the influence of erratics again prove too much, it would not take him half an hour to stamp an issue of his magazine with its correct date of publication.—ED., NAT. SCI.]

NORTH SEA FISHERIES.

IN our review of Mr. Holt's "Examination of the Present State of the Grimsby Trawl Fishery" (vol. viii., p. 52), we correctly quoted the price as 3s. 6d. We are, however, glad to learn from the Director of the Marine Biological Laboratory at Plymouth that reprints of this paper can now be had at 1s. each, either from Messrs. Dulau & Co. or from himself.

A CARD CATALOGUE TO ZOOLOGICAL LITERATURE.

DR. H. H. FIELD writes to us that this scheme is now in full operation, and states that "after February 15, the number of copies printed will be reduced so as to be but slightly in excess of the subscription list. Persons ordering much after that date run the risk of finding a part of the catalogue already out of print." Further information may be obtained from the Bibliographical Bureau, Universitätsstr. 8, Zurich-Oberstrass, Switzerland.

NOTICE.

TO CONTRIBUTORS.—*All communications to be addressed to the* EDITOR *of* NATURAL SCIENCE, *at* 22, ST. ANDREW STREET, HOLBORN CIRCUS, LONDON, E.C. *Correspondence and notes intended for any particular month should be sent in not later than the* 10*th of the preceding month.*

TO THE TRADE.—NATURAL SCIENCE *is published on the* 25*th of each month; all advertisements should be in the Publishers' hands not later than the* 20*th.*

THE "CHALLENGER" NUMBER.—*In reply to enquiries, we remind our readers that, although the* FIRST *edition of this ran out of print immediately, there are still some copies of the* SECOND *edition to be obtained at the usual price*—ONE SHILLING. *No more will now be printed, so orders should be sent at once.*